

ISRAEL IN TRANSITION

From Late Bronze II to Iron IIa (c. 1250–850 B.C.E.).
Volume 2. The Texts

(Conference Papers Supported by
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edited by
Lester L. Grabbe



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To

Jerry Lynn Garrison Sculley

in memory of growing up together

in Silverton, Texas

HU/i/2011-17440

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ABBREVIATIONS

AASOR	Annual of the American Schools of Oriental Research
AB	Anchor Bible
ABD	<i>Anchor Bible Dictionary</i> . Edited by D. N. Freedman. 6 vols. New York, 1992
ACEBT	<i>Amsterdamse Cahiers voor Exegese en bijbelse Theologie</i>
AEL	M. Lichtheim, <i>Ancient Egyptian Literature</i> . 3 vols. Berkeley, 1976–80.
AOAT	<i>Alter Orient und Altes Testament</i>
ARE	<i>Ancient Records of Egypt</i> . Edited by J. H. Breasted. 5 vols. Chicago, 1905–1907. Reprint, New York, 1962
BA	<i>Biblical Archaeologist</i>
BAR	<i>Biblical Archaeology Review</i>
BASOR	<i>Bulletin of the American Schools of Oriental Research</i>
BETL	<i>Bibliotheca ephemeridum theologicarum lovaniensium</i>
BN	<i>Biblische Notizen</i>
BZAW	<i>Beihefte zur ZAW</i>
CAH	<i>Cambridge Ancient History</i>
CBC	<i>Cambridge Bible Commentary</i>
CBQ	<i>Catholic Biblical Quarterly</i>
CIS	<i>Copenhagen International Series</i>
DJD	<i>Discoveries in the Judaean Desert</i>
EI	<i>Eretz Israel</i>
ESHM	<i>European Seminar in Historical Methodology</i>
FAT	<i>Forschungen zum Alten Testament</i>
H&T	<i>History and Theory</i>
ICC	<i>International Critical Commentary</i>
IDBSup	<i>Interpreter's Dictionary of the Bible: Supplementary Volume</i> . Edited by K. Crim. Nashville, 1976
IEJ	<i>Israel Exploration Journal</i>
Int	<i>Interpretation</i>
JARCE	<i>Journal of the American Research Center in Egypt</i>
JEA	<i>Journal of Egyptian Archaeology</i>
JESHO	<i>Journal of the Economic and Social History of the Orient</i>
JNES	<i>Journal of Near Eastern Studies</i>
JSOT	<i>Journal for the Study of the Old Testament</i>
JSOTSup	<i>Journal for the Study of the Old Testament: Supplement Series</i>
JSS	<i>Journal of Semitic Studies</i>
KHAT	<i>Kurzer Hand-Commentar zum Alten Testament</i>

LAPO	Littératures anciennes du Proche-Orient
LB	Late Bronze
LHBOTS	Library of the Hebrew Bible/Old Testament Studies
MB	Middle Bronze
NEA	<i>Near Eastern Archaeology</i>
NSK-AT	Neuer Stuttgarter Kommentar, Altes Testament
OBO	Orbis biblicus et orientalis
OTL	Old Testament Library
OTM	Old Testament Monographs
PdÄ	Probleme der Ägyptologie
PEQ	<i>Palestinian Exploration Quarterly</i>
SAHL	Studies in the Archaeology and History of the Levant
SBLABS	Society of Biblical Literature Archaeology and Biblical Studies
SBLDS	Society of Biblical Literature Dissertation Series
SBLSymS	Society of Biblical Literature Symposium Series
SHCANE	Studies in the History and Culture of the Ancient Near East
SJOT	<i>Scandinavian Journal of the Old Testament</i>
TA	Tel Aviv
ThWAT	<i>Theologisches Wörterbuch zum Alten Testament.</i> Edited by G. J. Botterweck and H. Ringgren. Stuttgart, 1970–
UF	<i>Ugarit-Forschungen</i>
VT	<i>Vetus Testamentum</i>
VTSup	<i>Vetus Testamentum, Supplements</i>
ZAW	<i>Zeitschrift für die alttestamentliche Wissenschaft</i>
ZBKAT	Zürcher Bibelkommentare. Altes Testament
ZDPV	<i>Zeitschrift des deutschen Palästina-Vereins</i>

LIST OF CONTRIBUTORS

A. Graeme Auld is Emeritus Professor of Hebrew Bible at the University of Edinburgh.

Marc Brettler is Dora Golding Professor of Biblical Studies at Brandeis University.

Philip R. Davies is Professor Emeritus of Biblical Studies at the University of Sheffield.

Lester L. Grabbe is Professor of Hebrew Bible and Early Judaism at the University of Hull.

Ernst Axel Knauf is Professor of Hebrew Bible and Biblical Archaeology at the University of Bern.

Niels Peter Lemche is Professor of Theology at the University of Copenhagen.

Robert D. Miller, II, SFO, is Associate Professor of Old Testament at the Catholic University of America in Washington, DC.

John Van Seters is Distinguished University Professor Emeritus, University of North Carolina at Chapel Hill.

INTRODUCTION

Lester L. Grabbe

This is the second of two volumes on the period ca. 1250 to 850 B.C.E. Volume 1 (Grabbe, ed., 2008) was on the archaeology, while the present volume focuses more on the textual sources, though some other sources are also looked at (cf. Grabbe, which surveys all the sources [pp. 62–129, below]). Most of the essays were discussed in preliminary form in the meeting of the European Seminar on Historical Methodology in conjunction with the European Association of Biblical Studies in Budapest (2006) and Vienna (2007). Partial funding for these meetings was provided by the Arts and Humanities Research Council (AHRC) of the UK.¹

When plans for the discussion in the present volume were originally laid out for members of the Seminar, there was considerable scepticism on the part of some as to whether such an investigation was possible. Archaeology was taken for granted as a potential way of understanding the history of this period, but were any biblical texts relevant? Many members of the Seminar were primarily textual scholars, whereas the archaeology was being discussed separately by a group of professional archaeologists. Was the discussion likely to produce anything of interest, or would it only confirm the uselessness of the text that had been concluded in some circles? There are of course some contemporary texts, especially Egyptian ones, but there are not many of these, and those that do exist are often problematic to use for historical purposes (see Grabbe for a survey of these). But the biblical text looms large in the discussions, though not all turned out to be as sceptical as the colleagues mentioned above (for a further discussion of the different views, see the “Reflections” chapter below).

1. The AHRC funds postgraduate training and research in the arts and humanities, from archaeology and English literature to design and dance. The quality and range of research supported not only provides social and cultural benefits but also contributes to the economic success of the UK. For further information on the AHRC, please see its website: www.ahrc.ac.uk.

Summary of the Volume's Content

NB: All of E. A. Knauf's contributions are summarized together, even though one appears as an Appendix to the present volume.

In his "Samuel, Sources, and Historiography," *A. Graeme Auld* poses the question: Need the sources of Samuel be early? He notes that scholars have commonly proposed sources for the various sections of Samuel; but wonders whether the very question "How was the book of Samuel composed?" may imply the (wrong) answer. It is not a matter of "components." For example, the story of David and Goliath differs considerably in the MT and the LXX. It is usually said that the more original LXX was expanded by a pre-existing source to give the MT. But an alternative suggestion is that the explanation is not in terms of rival sources but of natural growth in the narrative. In other words, the narrative grew midrash-like not by incorporating new sources, but by filling in gaps in the narrative from materials already at hand. For example, David's loyalty to the house of Nahash in Ammon (2 Sam 10) has provided the key category for a new narrative in 2 Sam 9 about David's loyalty to the house of Saul. Likewise, much of the story of the ark in 1 Sam 5–6 is a fresh re-combination of elements already present in 2 Sam 5–6, together with the temple of Dagon from 1 Sam 31. Two points can be made about the relationship between the material common to Samuel and Chronicles: one, that the version in Chronicles of the synoptic material is more conservative than the version in Samuel; two, that the shared text, in as far as it can be reconstructed, is our oldest and largest source for the books of Samuel and Kings. This shared text, "The Book of the Two Houses" (BTH), continues at least to the time of Josiah. The accounts of Saul, David, and Solomon were written as a preface to this royal history, drawing on it for inspiration. No source for David and Solomon is earlier than this narrative of the kings in Jerusalem, and the David–Solomon narrative is thus no earlier than the late monarchy.

Marc Brettler ("The David Tradition") begins his study by a lengthy survey of modern trends in historiography, concluding that a consensus is developing which rejects the extremes of postmodernism and scepticism, on the one hand, and those who privilege the biblical tradition and ignore the normal canons of critical historiography, on the other. In spite of its problems, the only course is a literary-critical analysis that makes no *a priori* assumptions about whether the text is historical or not. A careful examination of the parallel traditions in 1 Sam 24 and 26

concerning David fleeing from Saul offers a useful test-case for understanding the development of the David Tradition, and deciding how that tradition might bear on the historical reconstruction of the early monarchy. This paper attempts to use the canons of general historiographical study to examine these texts, their relationship, and their utility for the modern historian of Israel. It suggests that 1 Sam 24 knew and adapted the earlier 1 Sam 26, although ultimately ch. 24 is better integrated into the book of Samuel than ch 26. It also finds reason to believe that ch. 26, the earlier source, has reworked a now lost source. But in its present form the text of ch. 26 is still far removed from the age and life of David. Given the origin of these chapters, they may not be used to reconstruct the history of the early monarchy. The tendency of the chapters is very clear: it is to glorify the magnanimity and piety of David, while also condemning Saul for malevolence out of his own mouth. Even though it is true that all texts are evidence for something, we know too little about who may have composed these chapters, or when and why they were written, so they should not be used for reconstructing any aspect of the history of Israel.

Philip Davies ("The Beginnings of the Kingdom of Judah") examines the implications of the reference to *bytdwd* in the Tel Dan inscription. He accepts the authenticity of the inscription (though until it is properly authenticated a question mark hangs over any thesis depending on it), but the positioning of the fragments is still a moot point, as is the restoration at a number of points. The result is a number of interpretations. *bytdwd* is crucial to many interpretations, but does it mean "kingdom of Judah"? The Assyrian reference to "house of Omri" has often been invoked for this interpretation, but the Assyrian inscriptions refer to "Judah," not "house of David." Of the 25 biblical references to "house of David" in the Bible, none means "kingdom of Judah." Some refer to a building/dwelling, but most refer to "the family of David" or the ruling dynasty. Lemaire's restoration of "David" in the Moabite inscription, even if it is correct (a number of epigraphers disagree with this reading), doubtfully means "kingdom of Judah." It is unlikely that the Tel Dan stele refers to a kingdom or state of Judah alongside Israel; on the contrary, the kingdom of Judah does not exist in the Tel Dan inscription. Whether there was a state centred on Jerusalem is unclear, but the biblical text indicates Judah in the 9th century was controlled by Israel. There was a "united monarchy"—ruled by the Omrides. So, even though *bytdwd* might yet be read as a reference to a vassal chiefdom of the kingdom of Israel, it remains to be argued that this chiefdom can be identified as a kingdom of Judah, or its leader as *melek*.

Lester Grabbe ("From Merneptah to Shoshenq: If We Had Only the Bible...") continues to ask the question asked in a number of earlier studies: If we had only the Bible, what might be known about Israel's history? Unlike the time from the mid-9th century when many inscriptions are available, however, there are few extra-biblical sources in the earlier period (ca. 1250–850 B.C.E.). This makes the problem of testing the biblical narrative more difficult. When we look at the biblical narratives that purport to cover this period of time (mainly Exodus, Numbers, Joshua to 2 Kgs 11), we find little to corroborate them, at least until we come to the narratives of Saul and David. By and large, the archaeology does not support the main biblical narrative of the exodus, wandering in the wilderness, and conquest of the land. When we come to Saul, David, and Solomon, many scholars have thought we had reached a secure period of biblical history. Again, though, the archaeology does not give firm support. The Saul and David narratives may have a historical core, despite many problems, but the Solomon story looks mainly legendary. Here and there may be reliable data in the text, but so far the investigation does not suggest there are many such data. The impression gained from earlier studies seems to be borne out: the later the history of the monarchy the more reliable the text seems to be.

Axel Knauf ("History in Joshua") asks: How much "history" found its way into the present text of Joshua and also what kind of history? Accepting the historical-critical analysis of biblical literature as basically valid, he surveys the literary history of Joshua: late 7th century (first draft, end of an Exodus-Conquest Story: Josh 6* and 10*); 525–450 (Exodus-Conquest Story becomes D-Composition: Josh 6–11*); 450–400 (first Hexateuch redaction: Josh 1–11*; 15:20–18:1*); 400–375 (second Hexateuch redaction [Josh 13; 20–22; 24] and Joshua-Judges Redaction [Josh 12; 14; 18–19; 23]). There are memories of cities that were seats of rulers in the LB II and some other data. Those from the earliest through the 9th century are as follows:

1. 13th: Josh 15:9; 18:15 ("waters of Nephtoah" = "spring-place of Mer-en-Ptah").
2. 13th/12th: Josh 17:2, 14: Manassite clan from survivors of Merneptah's Israel.
3. (a) 10th: Josh 10:1–14* (10:12c–13c) (battle of Gibeon).
(b) Josh 15:63 (Jebusites still dominate Jerusalem even after David).
4. 10th/early 9th: Josh 10:13d ("Book of Jashar/the Upright").
5. 10th/early 9th: Josh 16:10; 17:11–13 (Canaanite cities integrated into Israel).

Quite a few other passages have historical references, but these relate to the 8thth to 3rd/2nd century B.C.E. Thus, most texts with references to datable events are post-Iron IIA. Taking all the passages—both early and late—the features from Joshua are evaluated as "possible" or "probable" (the difference between the two being mainly one of chronological precision). The probable/possible features tend to cluster in the pre-exilic period (which is a long one) and the Persian period, with few in the pre-state and Hellenistic periods. That is, the "historical memory" of Joshua tends to focus equally on the pre-exilic and the Persian periods, with the pre-state little remembered and the Hellenistic coming mostly after the completion of Joshua. The oldest piece of text is quoted from the 10th century (Josh 10:13d); larger amounts of text are preserved from the royal administrations of Israel and Judah in the 9th and 8th centuries. The fundamental expectation that the text should reflect best the time of its authors (rather than the time about which they wrote) is fulfilled in that the later features have more text and precision; however, the historicity rate of early literary strata is not necessarily higher than that of later strata. Thus, one cannot read a core narrative uncritically nor ignore possible historical information in redactional additions. Joshua has preserved elements of historical information, but the historical narrative it gives does not correspond with historical reality.

Axel Knauf continues his investigation in "History in Judges." Less than 25 percent of the book's contents derive from the 10th to 9th centuries B.C.E. and the LB/Iron I, though almost nothing from the latter. The pre-exilic core grew by adding layers, with the "Book of Saviours" constituting the first addition in response to the loss of the Israel's king. A Jewish adaptation, turning the anti-monarchic, anti-state attitude into pro-state propaganda, began in the Persian period. A number of passages potentially have a historical basis; of these the following could have come from the 9th century or earlier:

- 3:12–30:
The defeat of Eglon by the Benjaminite clan Ehud was possible during the period of weak state power, either ca. 1100–875 B.C.E. or 724–716 B.C.E.
- 3:13:
Moab's conquest of Jericho was possible once Mesha had conquered Nebo ca. 850/840 B.C.E.
- 3:22–23:
This presupposes an Assyrian type of toilet, which was possible from 716 B.C.E. onwards, with the Assyrian palaces at Samaria and Megiddo.

4–5:

The Song of Deborah was not composed before the 10th century B.C.E. and first committed to writing in the Omride court (ca. 875–850 B.C.E.). It describes a conflict between the tribes of Zebulon and Naphtali, on the one hand, and the Sea Peoples leader Sisera, on the other. Many features of the poem are the common stock in trade of professional singers, while the prose account (ch. 4) is derived from the Song.

8:3–21:

Gideon's eastern campaign may reflect the Midianite involvement in the Rift Valley copper trade from the 11th to 9th centuries (LC). A conflict between the clan of Abiezer and a Midianite raiding party is possible anytime during this period.

8:10:

The Israelites fought a battle with the Assyrians at Qarqar in 853 B.C.E.

8:13–17:

Pharaoh Shishak conducted a campaign that destroyed Penuel and Succoth, though Jeroboam I then rebuilt Penuel.

9:

Shechem was not settled between the late 11th and the late 10th centuries (LC) but was resettled as an Israelite royal residence. The “Abimelech/Shechem” tradition is thus unlikely to predate 1050 B.C.E., but it seems to describe tensions between tribe and town, perhaps David and Jerusalem or even Saul and Gibeon.

The historical memory in Judges is found in texts that are often much later than the events. In collective memory anything over 200 years old is “distant past”; 100 to 200 years ago represent the “past”; while things less than 100 years ago are part of the “present.” The examples can be divided into “probable” events (total of 11) and “possible” events (total of 12). Of the *probable* events, 45 percent fall into the 100 years past, 27 percent into the 200 years past, and another 27 percent into the “distant past.” Of the *possible* events, 25 percent fall into the “present” within the past 100 years, 42 percent in the “past” up to 200 years before, while a full 33 percent were of the distant past of more than 200 years earlier. There were a number of discontinuities between 1000 and 720, which resulted in breaks in the collective memory. For example, the Omride history has almost no representation in Judges, and of the rich literature only the song of Deborah is preserved in the biblical text.

In his final piece (Appendix), *Axel Knauf* reflections on history with regard to the “Exodus and Settlement.” The idea that Yhwh led Israel out of Egypt is already present at the foundational stage of the biblical

tradition, but the view that he led Israel into Canaan is no earlier than the 8th century B.C.E., while it is in the theology of the 7th to 6th centuries that the land becomes a gift from God. It is probable that in the 12th century a group of refugees, descendants of prisoners of war taken by Mer-en-Ptah, arrived in Canaan with the message that “Yhwh led us out of Egypt.” The Exodus creed may have entered the state cult of Israel by the reign of Jeroboam I (end of 10th century), though more likely the reign of Jeroboam II (mid-8th). Moses' Egyptian name suggests he was associated with the Exodus tradition from the beginning, but his biography has remarkable parallels with that of Jeroboam I and was probably modeled on it. The “baby in the bullrushes,” however, was probably derived from the legend associated with Sargon I, under the influence of the Akkadian Sargonid dynasty. The entry into the land was not an important tradition as long as the nation of Israel existed, but when the people ceased to be a state (though still in the land), Hosea emphasized the Jacob and wilderness traditions. But now the question arose as to who the people were who lived in the land before Israel and what had happened to them. Assyrian theology, centring on subjugation and vassal treaties with subordinate peoples, was very influential. The list of peoples allegedly driven out was derived from the “academic” knowledge of the writers. For example, the Amorites were known from Babylonian texts but probably via Aramaic versions of documents available in Jerusalem. Traditional and even mythic names found their way into the narrative. The concept of annihilation of the native inhabitants seems to have come in with the Assyrians: the *h̄erem* texts belong to the D-expansions of the book rather than the original narrative. The proposed genocide of the original inhabitants had only an “encyclopaedic” existence. Those to be driven out or struck down in the context of D were actually the Benjaminites—an inner-Jewish conflict has been projected onto external foes. The events of the “historical” narrative became part of preserved memory and are celebrated anew each year; the collective memory of the yearly cycle has become a book. The failed history of the monarchy has been neutralized but not forgotten, lest the error be repeated.

Niels Peter Lemche has doubts about whether an examination of history during Iron I and Iron IIA can discover anything of interest (“How to Deal with ‘Early Israel’”). He organizes his discussion around five questions: (1) *What happened in the way of writing a history of the period?* He mentions two works: the revised Miller/Hayes and the recent work of Liverani. The former unfortunately takes a “middle-of-the road”

perspective, which invites being attacked from both sides. It will also produce little new knowledge, but Liverani is more innovative. (2) *What happened from an archaeological point of view?* The recent extensive archaeological excavations have not generally been pertinent to the period of the 13th to 10th centuries B.C.E. The work in Tel Aviv has met little challenge (except from Jerusalem), and we are a far cry from the earlier politically inspired biblical archaeology. The creators of the modern state of Israel chose history as a vehicle for nationalism. The Marxist rebels of 1968 chose history as an object of hate. Archaeology has not changed much in the past ten years: Finkelstein's outline of twenty years ago still holds. The most innovation relates to the later period: O. Lipschits on the 7th to 5th centuries B.C.E. (3) *What happened within social anthropology to help in reconstructing early Israel?* It is the North American tradition of social anthropology that has most influenced archaeology. Cultural evolution is misleading. It was examined in *Early Israel*, but the mistakes of the past keep being repeated (e.g. in S. Cook's *Social Roots of Biblical Yahwism*). Material culture does not necessarily conform to ethnicity. Dever has deservedly been attacked for assuming this, but A. Killebrew could also be criticized. R. D. Miller's work has much to commend it (cf. pp. 167–98, below), but it examines only one model among many. When it comes to incorporating sociological analysis into historical studies, not much has happened. (4) *What happened in the way of reading biblical texts that would allow for new ideas of history and historicity?* Lemche's *Canaanites* and Thompson's *The Bible in History* found more acceptance among literary students than biblical historians. The Copenhagen School is not postmodern but modern, but it has to recognize that if the relativism of postmodernism wins, it will be the death of history as a discipline. The intellectual mood of today does not advance historical interests. Most modern literary theory applied to the Bible is misleading. This is because of a total change of aesthetic norms since antiquity. But it is doubtful that we can escape modern hermeneutics and continue to be biblical scholars. This leads us to present "twisted" readings of biblical and other ancient Near Eastern texts. Then ancient historiography becomes interesting because it tells us what people believed to have happened. (5) *What kind of history can be written?* Liverani's work was divided into two sections. The first ("normal history") was more traditional than might be expected from him, but the second part ("invented history") centres on the meaning of texts: Why were they written? He shows that we do not get from history to ideology but from ideology to history. It is possible to write the history of Palestine without the Bible.

Robert D. Miller II discusses "A 'New Cultural History' of Early Israel." The first part of the essay surveys—mainly by a series of quotations—some recent views about writing a cultural history of Israel. Several concrete issues will be grappled with, the first being house design. The classic "four-roomed house" is typical of the Iron I highlands but not unique to that region. The Bunimovitz/Faust analysis is critiqued, though no generalizations about town design are offered. Gender division of tasks is well known in most pre-modern societies and is indicated for Iron I Israel. Settlement patterns as reconstructed by archaeologists differ from the tribal allotments of the biblical text, which seem to be based on later tribal boundaries in the region. The settlement maps can be compared with artifact distribution maps. Several pottery types are excluded for practical reasons, including bowls, jugs, lamps, and also cooking pots. Storage jars are potentially useful, and their distribution pattern is plotted (though there is a debate over whether they could be transported or not). But the entire typological method of Syro-Palestinian archaeology is to be questioned, since "R-Analysis" is now thought preferable to "Q-Analysis." The question is asked whether topological analysis is a good way to culture—which would negate the discussion of the first part of the article! Texts are also a cultural artifact. Only a minimum have been found for Iron I—found mainly in small, obscure sites (which itself raises issues). Evidence of religion is found mainly in cult sites, but the evidence is very meagre: the famous "Bull Site" is excluded as an Iron I shrine. Some known sites (e.g. at Dan) are outside the central hill country. The Mt. Ebal "altar" might be cultic. The metal bull associated with the "Bull Site" was not found by archaeologists and is probably MB; otherwise, no evidence for the cult is known for this period. There are some Iron I figurines but even more incense burners (probably, though not certainly, cultic). Mortuary evidence is also skimpy, but there is no evidence for a warrior cult. Some graves have pottery only but others have other sorts of grave goods as well. The new Iron I highland settlements indicate new and different reactions to the cultural meaning imposed on the landscape by the LB inhabitants. The biblical depiction of early Israelite religion is "accurate" in indicating religious practices that do not accord with the standardized religion of later times.

John Van Seters ("David the Mercenary") points out that in the Davidic narrative David shows considerable reliance on mercenaries in his time; on the other hand, there is hardly any mention of them in the later narratives of Kings. This suggests that the presence of mercenaries might provide an indication of the time and extent of a late revision of the

Davidic narratives. First the question of the use of mercenaries in Greece and the ancient Near East is investigated. From the time of Tiglath-pileser III trained soldiers of conquered peoples were coerced into serving the Assyrians. Psammeticus I (ca. 650 B.C.E.) was apparently the first to use Greek mercenary soldiers, to fortify the eastern Delta. The Saite dynasty seems to have used a variety of mercenaries, including Jews, and Greek mercenaries appear to have been stationed at Mesad Hashavyahu and Arad. This employment by the Saites increased significantly the number of Greeks who became mercenaries, understandable in the light of the lack of resources to maintain the Greek population by agriculture alone. The conquest of Egypt by the Persians halted the use of Greek mercenaries for a time, but the practice began again at the end of the 5th century: Cyrus the Younger used mercenaries. However, it is only the use of Greek mercenaries in this later period that is relevant for the David story. In the David story, David himself captains a band of mercenaries in the employment of the Philistine king of Gath (1 Sam 27; 29). They live on the booty taken on their raids. Throughout the narrative David and his men are recognized as highly trained professionals. After David became king he continued to use a professional army to do his fighting, according to the late David Saga (in contrast with DtrH, which has David lead his own citizen conscripts). Benaiah, the commander of David's Greek mercenaries—the Cherethites and Pelethites—is a serious rival to Joab, commander of the regular forces, and eventually replaces him (2 Sam 8:18; 1 Kgs 1–2; 4:4). This account of the Davidic monarchy fits the militaristic regimes of the late Persian period because it is only from the 4th century B.C.E. onwards that Persian rulers and satraps made use of these particular Greek professional mercenaries, the Cretans and the peltasts, with their specialized skills. In conclusion, the references to mercenaries in the David Saga are so pervasive that they cannot be removed by redaction-critical methods, and the narrative cannot be “read against the grain” to extract historical information, because the narrator has freely invented a portrayal of David modeled on the monarchs of his own day. One cannot use any of it to reconstruct the Davidic monarchy of the 10th century.

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Part II

ESSAYS

SAMUEL, SOURCES, AND HISTORIOGRAPHY

A. Graeme Auld

Introductory

To understand the history of the development of the materials in the book of (1 and 2) Samuel is largely to remove Samuel from the list of potential sources for writing the history of the Levant in the earlier Iron Age. And yet to make such a claim is to fly in the face of what has passed as common wisdom for many generations of scholarship. A more familiar opening statement might go as follows: Historians work with sources. The book of Samuel is history (of a sort, at least). Searching for the sources of Samuel is a sensible and proper thing to do.

Asked about how the book of Samuel was “composed,” many Hebrew Bible scholars will start by identifying discrete sections, and arguing for at least relatively greater original independence for some of these: the birth and call of Samuel (1 Sam 1–3); the ark narratives (1 Sam 4–6; 2 Sam 6); the debate about kingship (1 Sam 8–12); Saul before David (1 Sam 13–15); Saul and David (1 Sam 16–31); David as king (2 Sam 1–8); and so on. They may not be sure about lines of demarcation, but will argue that these sections could have had a life of their own before they were used as they are now. It has been suggested by Thomas Römer (2005) that at least some such collections were part of Josiah’s library.

Many scholars will also draw attention to doublets, especially in 1 Samuel: varying accounts of how David came to the attention of Saul (1 Sam 16–18); David’s double flight from Saul to Achish of Gath (1 Sam 21 and 27); Saul twice in David’s power, and spared by him (1 Sam 24 and 26—see also Brettler’s contribution to the present volume); differing accounts of Saul’s death (1 Sam 31 and 2 Sam 1). Such doublets are held to represent alternative versions drawn from some of the source-material available to those responsible for the book as we know it.

And yet: there is a danger that the answer given is already implicit in the way the question is asked: that “composition” implies “components”—that it is only possible to “compose” by assembling pre-existing

"components". Historians work with sources. Some of these fit better, and some less well, with the overall view which the historian wants to express. It is often claimed that biblical "historians" demonstrated their integrity by coping with, rather than suppressing, variant traditions at their disposal.

Variant "views" (even if not "traditions") are frequent in the material. The book of Samuel is at odds with itself over whether kingship was a good thing for Israel—and, more specifically, whether David was a good thing or not. There is in Samuel a high view of kingship, and a correspondingly high view of David. And these mirror the high view of both David and kingship which we find also in the Psalms and in Chronicles. Within Samuel, this high view is most obvious in such poems as the Songs of Hannah (1 Sam 2:1–10) and of David (2 Sam 22) and the Last Words of David (2 Sam 23:1–7), as well as in the narrative in 2 Sam 7 concerning the oracle of Nathan about David's everlasting dynasty, and the following prayer of David. Throughout the book of Kings too, just as in Chronicles, it is David (and not the proverbially wise Solomon) who provides the high standard by which his successors in Jerusalem are judged.

The prime example of the opposite view within Samuel is the account of David's adultery with Bath-Sheba and the murder of her husband Uriah, and the resultant oracle of Nathan that the sword would never leave David's house (2 Sam 11–12). A lawyer may complain that a contrived killing should not be described as murder. However, in the analogous case of the contrived killing of Naboth, Elijah is instructed (1 Kgs 21:19) to sum up the situation in just two Hebrew verbs: "Have you murdered and also taken?" The book of Kings does once admit (1 Kgs 15:5) that the record of David as good exemplar of royal behaviour was limited by this case involving Uriah and his wife. However, the book of Samuel seems to go much further: it is at least open to the interpretation that the David–Bathsheba–Uriah affair was far from a lone exception, and was simply the worst example of characteristically manipulative behaviour by David which did not correspond to high norms. The book of Samuel does embrace different views of kingship and of David—and it is a matter for historians to decide whether one is earlier or later than the other, or whether they were simply contemporary.

Comparing Text with Text

The story of solitary combat between David and the gigantic Philistine champion is exceptional in two important ways. It is the only instance in the Hebrew Bible of stalemate between armies settled by a heroic wager

between individual champions. And the MT and LXX^B differ more substantially here than in any other part of the Bible; and this textual puzzle is all the greater because the materials found at Qumran are of almost no help (a tiny fragment gives support to MT at 1 Sam 18:4–5). The biggest single element in the difference between the two versions is represented by a substantial narrative of 20 connected verses (1 Sam 17:12–31) present in the longer Hebrew text, but not in the shorter Greek. This tells of the shepherd boy who was sent to visit his brothers on campaign, and who learned there about Goliath's challenge and the prize offered by his king for disposing of that challenge, and whose conversation led to his being brought before Saul. Even scholars who are persuaded that the shorter Greek version of 1 Sam 17–18 is witness to a shorter Hebrew text older than MT (Lust 1986 and Tov 1986) tend to argue that these added verses were drawn from a previously existing source: the "composer" of the longer text inserted a pre-existing "component".

In a couple of studies (Auld and Ho 1992; Auld 2004b), I have argued an alternative case: that the fresh introduction of David to Saul on the battlefield was written for its present context—and that most of its information was drawn from the existing shorter text. Indeed, my suspicion is that, in one very important respect, 1 Sam 17–18 is not exceptional at all: it just happens to be a very rare case where clear evidence has been preserved of how portions of the book of Samuel were first written and then rewritten. Alternative versions of David and Achish, of Saul in David's power, and of Saul's death, are not to be explained in terms of rival sources or alternative versions combined (against Brettler in the present volume), but of narratives written and then rewritten. The substantial new writing—or re-writing—demonstrated in the longer version of 1 Sam 17–18 documents the latest example of this process, and indicates a sensible way to understand the earlier writing and re-writing which produced other doublets.

Earlier stages in the authorial process can similarly be deduced from other sorts of comparison of real (and not merely hypothetically reconstructed) texts, for example by comparing Samuel with Chronicles. The synoptic or shared account of David's loyalty (*hesed*) to the house of Nahash in Ammon (2 Sam 10) has provided the key category for a new narrative in 2 Sam 9 about David's loyalty to the house of Saul. Much of the story of the ark in 1 Sam 5–6 is a fresh, anticipatory re-combination of elements already present in (synoptic) 2 Sam 5–6, together with the temple of Dagon added from 1 Sam 31 (the first synoptic chapter). Several stories about the sin, or guilt, or folly, of Saul or David are anticipatory, midrash-like, attempts to explore some of the puzzling gaps in

the story of Yahweh, David, and the counting of Israel in 2 Sam 24 (Auld 2002). Key categories from the royal ideology in 2 Sam 7 (such as *nāgîd* and “my people Israel”) are anticipated again and again in the writing of 1 Samuel. The summary of Saul’s relations with his neighbours (1 Sam 14:47–48) is drawn from the summary of David’s relationships in (synoptic) 2 Sam 8:10b–12.

It May Be Possible, But is It Wise?

A case *can* be made for deriving much of 1 and 2 Samuel from the relatively few materials shared by Samuel and 1 Chronicles, that is, most of 1 Sam 31; 2 Sam 5–8; 10; 24; plus 1 Chr 20 and the traditions connected with David’s three and thirty heroes. But is it compelling? Many of my colleagues do not think so; and a substantial critique of my position has been offered by John Van Seters (2007). It appears that some of my earlier statements had been incautiously brief, and hence open to misunderstanding. I have never sought to defend the position that the authors of the (present text of the) book of Chronicles had no knowledge of Samuel–Kings beyond the synoptic materials. That would be most implausible. But I do hold two things to be arguable: (1) that the version in Chronicles of the synoptic material is more conservative than the version in Samuel (more conservative even than the older forms of the text of Samuel available to us from 4QSam^a and OG); and (2) that the oldest form of the shared text, in as far as it can be reconstructed, is our oldest and largest source for the books of Samuel and Kings.

One example of such conservatism may clarify the position. The version in 1 Chr 14:3–7 of the list of sons born to David in Jerusalem starts: “And David took more wives in Jerusalem.” The version in 2 Sam 5:13–16 reports the addition of “more concubines and wives” (MT and 4QSam^a) and “more wives and concubines” (LXX^B). Typically, the added word “concubines” has been added in a different position to different texts of Samuel. (It should be noted that Cross et al., in their discussion of 4QSam^a [2005, 122], claim instead that 2 Sam 5:13 once read “concubines” minus “wives”.) First Chronicles 3:1–9 has brought together the account of David’s Jerusalem sons from this synoptic text with the special Samuel material about his earlier Hebron family (2 Sam 3:2–5). There Chronicles does add mention of David’s concubines (but in a different position again), and of his daughter Tamar. This fuller list in the genealogical prologue to the book of Chronicles makes it plain that there would have been no ideological reason within the book of Chronicles for concubines to be deleted from 1 Chr 14—they simply had not been

there in the source (against Cross et al. 2005, 122). We might expect that Hebrew *plgš* (“concubine”), vocalized as *pileges* in MT, had been vocalized earlier as *palagš*. There is little doubt that this word was related to Greek *pallax/pallakis/pallakē*. Although Classical and Semitic philologists disagree over the direction of influence, it is at least arguable that *plgš* belonged to later biblical texts—and, if so, was a Greek import.

Evidential Value?

If the story of David and his rivals and predecessors was written and many times re-written, as I have suggested, the value to the historian of most of the book will rest almost entirely in what we can learn about the interests of the period or periods of rewriting. I have in mind, for example, the analysis of Goliath as a champion fitted out in Greek armour (Rofé 1987), and recent work about the performance in the Hellenistic period of selections from Greek tragedies at symposia as a reservoir on which elements in the Saul narratives have drawn (Adam 2010).

If the major source—and the oldest substantial source—of the book of Samuel is the material shared with Chronicles, then we must examine what I have been calling for some years “The Book of Two Houses” (Auld 2004a, 3–9; hereafter BTH). This material common to Samuel–Kings and Chronicles is of two different sorts. The second half covers most of the Davidic kings who ruled in Jerusalem; and underlies 1 Kgs 12–2 Kgs 25 and 2 Chr 10–36. Many of the kings are dealt with quite briefly, but a few at somewhat greater length. The length of reign, the death of each king and his burial (mostly) in Jerusalem, and the succession of (mostly) his son is reported in formulaic manner. And it is this material which is closest in form to Assyrian records (Grabbe 2007). These reports have a higher likelihood than any others of having been based on royal records—those from Jerusalem. I make this broad claim in contrast to two other blocks of biblical material about kings.

The first is the material peculiar to the book of Kings (and not represented in Chronicles) about the kings of (northern) Israel. The synchronistic interleaving of this material with the record of the line of David in Jerusalem follows an established ancient Near Eastern pattern. However, that comparison need offer no encouragement to considering the subject matter itself as having been drawn from a similar primary source. It is much less likely that, as a whole, it had a close relationship with royal records in Samaria. Nine of the northern kings (Jeroboam, Baasha, Omri, Ahab, Joram, Jehu, Jehoahaz, Jehoash, and Pekah) were already mentioned within the Jerusalem records just discussed; and the historicity of

at least two of the ten others (Menachem and Hoshea) is guaranteed by Assyrian inscriptions. However, the record as a whole is secondary (Auld 2007): it is wholly implausible that David and Solomon should have had exactly the same number (nineteen) of royal successors in the north as in Jerusalem, and that one wicked woman should have had so important a role in exactly the same position in each list (Jezebel and Athaliah, both in seventh place). The succession of Israel's kings was created for the biblical book of Kings. Seven kings, not one of whom is mentioned in the Jerusalem record or in Assyrian inscriptions, are said to have ruled for two years or less. It seems very likely that the scanty and stylized reports on at least some of these have served simply to make up the numbers. By contrast, the annotated list of nineteen successors to Solomon in Jerusalem has much higher plausibility as a source, or part of a source, of the book of Kings. Lamb's identification of many more correspondences between the biblical accounts of Israel's and Judah's kings points in a similar direction (see Lamb 2007).

The second block of contrasting material is the first half of the BTH. Much more extended narratives about David and Solomon occupy that space than are provided for any of their successors: roughly the equivalent of eight biblical chapters on each king (1 Kgs 3–10 on Solomon, and see above for David). These narratives about the first kings of the Davidic house perform a number of functions within the BTH. They provide a sketch of “first things” in two senses of that term: of specific Jerusalem “beginnings,” and also of larger “principles”. Heavily charged narratives such as Nathan’s dynastic oracle and David’s response, such as Yahweh having David count Israel and Yahweh’s response to that census, such as Solomon’s two visions and his extended prayer at the dedication of the temple—these narratives articulate the principles and pose the questions which underlie the whole BTH, a work which had attempted nothing less than a theodicy of Jerusalem’s collapse.

Unlike what follows in the BTH, this paradigmatic account of Israel’s first kings in Jerusalem was not material from royal records. It had instead been composed as an interpretive prologue to the following material (which had been drawn from royal records) on the subsequent kings. That is not to say that David and Solomon, and Saul before them, never existed; but it is to suggest that the royal records of Jerusalem in the late monarchical period may have contained no material on David and Solomon of the sort that they possessed for the later kings. Unlike all their successors, both David and Solomon are credited with reigns of 40 years, a typical ideal round figure. To judge from the different sorts of material in the two halves of the BTH, David and Solomon inhabited mythic rather than historical space.

If these first accounts of David and Solomon were written as preface to the royal record of Jerusalem, then where they share details with what follows, we need to read from end to beginning and assess the fresh preface in the light of the older material which now follows them. I offer just one example of historical and archaeological interest. The short report of David’s occupation of Jerusalem (2 Sam 5:6–10) equates two topographical terms. One of these, *msdt sywn* (“the stronghold of Zion”), which he captured, is otherwise unmentioned in the HB. ‘*yr dwd* (“the city of David”), by contrast, names the royal necropolis throughout most of the second half of the BTH: king after king is buried there (1 Kgs 14:31; 15:8, 24; 22:51; 2 Kgs 8:24; 9:28; 12:22; 14:20; 15:7, 38; 16:20 [altered in 2 Chr 28:27]). It must have been the use of ‘*yr dwd* in connection with the burial of most of the Davidic kings which set the meaning of the term for the first readers of 2 Sam 5, rather than the other way round. When the author of the David and Solomon materials at the beginning of the BTH wrote of David taking possession of what came to known as “the city of David,” of David moving the ark there, and of Solomon moving the ark out of there into the new temple, then the author was thinking of “the area of the royal tombs”. Only the royal necropolis (city of the dead) quarter merited that name.

There is of course an analogy between the argument of this essay and the proposal of Joseph Blenkinsopp (2002) to see the J-material in Gen 1–11 as a lay supplement to a more original Priestly account of beginnings. In study of Samuel–Kings, this more formal material is often termed “Deuteronomistic” and is generally understood to constitute the later editorial layer. In both cases the more formal, official-theological material becomes viewed as the *Grundschrift*, while the great stories are later additions, complements, and subversions.

Linguistic Control?

This, then, is a sample of my account of the origins of the BTH—and with it the origins of the books of Samuel and Kings. Of course, there are rival positions, yet deciding between these is often little more than a matter of personal or group preference for alternative “readings” of the evidence. We lack adequate data to give us sufficient control of the few data which are available to us. It is entirely reasonable that others will have a *preference* for different explanations. But what sort of *argument* would invalidate this account?

One objection often advanced is “the known history of the Hebrew language”: the book of Samuel cannot be conceived of as a post-monarchic product—it is a stock example of Early or Standard Biblical

Hebrew, by contrast with Chronicles, which is a stock example of Late Biblical Hebrew. However, as was argued in papers by Robert Rezetko and Ian Young also delivered at the SBL/EABS Meeting at Vienna in 2007 (see Rezetko 2009; Young 2009), this familiar argument turns out on closer inspection to be completely circular (for earlier contributions by both, see Young, ed., 2003). They have since co-operated in preparing a very substantial fresh examination of the issues (Young and Rezetko 2008).

Are There Benefits?

(1) Arguments quite distinct from her own are advanced in support of Barbara Green's brilliant analysis of 1 Samuel as a fictional essay critical of kingship (see Green 2003).

(2) The problem of the plus in 1 Kgs 15:5 vis-à-vis Chronicles about the Uriah affair is solved at a stroke. David as exemplar for all his successors throughout the book of Kings was inherited by the author of Samuel from the BTH. In that earlier work, David's principal fault was to do what Yahweh had encouraged him to do, to count his people (2 Sam 24). Much of the book of Samuel is designed to subvert this exemplary inheritance; and the book of Kings takes an early opportunity to acknowledge (as if in a footnote) the most notorious exception reported in the new book of Samuel to David's earlier reputation.

(3) More important: this fresh text-based, historical criticism poses fresh questions. How, for example, should the historian seek to connect and account for the following observations of the textual historian? (a) The summary of Saul's victories (including supremacy over Amalek) is derived from the summary of David's. (b) Amalek is the only foe in the David summary about whom no further information is provided in the BTH. (c) The extended narratives about Saul and Amalek (1 Sam 15) and about David and Amalek (1 Sam 30 and 2 Sam 1) are all within rewritten doublets. It appears that, for one or more creative writers, a lone summary note had provided freedom and opportunity. But why had the BTH mentioned Amalek, uniquely among David's enemies, in only summary terms? And why was it about this same Amalek (or was it the same?) that the latest contributors to the book of Samuel added such substantial narratives?

To answer the question I set myself in this essay: it seems unlikely that any source of Samuel is earlier than the annotated list of (all or most of) the successors of David and Solomon in Jerusalem (the oldest source of the book of Kings). It remains open to debate whether that list had

included the final few kings before the ultimate collapse of Jerusalem (whether its compilation was late monarchic or exilic); but it had extended at least as far as Josiah. The narrative about David and Solomon prefaced to it will have been crafted no earlier than the last years of the monarchy.

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THE DAVID TRADITION

Marc Zvi Brettler

I. Introduction

I am not yet ready to discuss the David Traditions as a whole, but am here offering a small test-case; I believe that the example I have chosen will begin to illuminate some of the broader issues as well. I will look at the two parallel stories at the end of 1 Samuel, where David is fleeing from Saul, had the opportunity to kill him, but refuses to kill **משיח יהוה**, “YHWH’s Anointed.” These episodes, covering at least 1 Sam 24¹ and 26,² have played a central role in biblical scholarship: Spinoza already recognized them as a doublet in his *Tractatus* (Spinoza 1991, 176), and Wellhausen suggested that these are “two versions” of the same story in his *Prolegomena* (Wellhausen 1973, 264).

I will set the stage for the study of these two chapters by clarifying my understanding of how historians, both general and biblical, should function, especially how they should use textual evidence. I will then examine the treatment of these chapters in various histories of Israel, commentaries, and similar works. Finally, I will use these different pieces of background to explain how I would use these chapters to reconstruct history, and the implications that this might have for understanding the David traditions more broadly.

1. There is significant debate about whether or not a new unit begins with 24:1 or with 23:14 or 19. Thus, “1 Sam 24” will function as shorthand for 23:14 or 23:19 + ch. 24 or 24:1–24:23.

2. In this essay I am examining chs. 24 and 26 as they have been preserved, though it is quite possible that there are layers of composition and secondary elements in each. See Koch 1969, 141–42 and Cryer 1985. I am also certain that, as elsewhere, there is some redactional activity to accommodate each of these chapters in its current context; for a discussion of this issue in a different context in Samuel, see Willis 1973.

The purpose of this essay is methodological, so I will try to be as clear as possible about my presuppositions and logic. I am here following the practice advocated by Kratz, *The Composition of the Narrative Books of the Old Testament*, whose preface opens:

This book offers an introduction to the narrative works of the Old Testament, as far as possible without presupposing any hypotheses, simply on the basis of some undisputed fundamental assumptions like a distinction between the Priestly and the non-Priestly text in the Pentateuch, the special position of Deuteronomy... (Kratz 2005, ix)

Of course, some biblical scholars would dispute the “simple” distinction that Kratz makes between “hypotheses” and “undisputed fundamental assumptions.” I believe that this is a useful distinction, though I would rather view his “undisputed fundamental assumptions” as “extremely likely hypotheses that have stood the test of time.” In the following section I will adduce what I believe to be several “undisputed fundamental assumptions” or “extremely likely hypotheses that have stood the test of time” concerning the nature of historical writing.³ In the same manner that some scholars might disagree with Kratz about “the special position of Deuteronomy,” some will disagree with aspects of my description of the current status of historiography. I will do my best to make all of my assumptions as explicit and clear as possible, so that these assumptions, as well as their application, are open to scrutiny and discussion.⁴

II. Historiography in Crisis⁵

A. General Historiography

The title of Iggers’s book, *Historiography in the Twentieth Century: From Scientific Objectivity to the Postmodern Challenge* (Iggers 1997), captures perfectly the changes in attitude from early in the 20th century through the end of that century. The last three decades in particular saw some deep skepticism about the ability to recover the past, and about what it means to write history.⁶ I believe that there is movement toward

3. Many of these build upon my *The Creation of History in Ancient Israel* (1995) and “The Copenhagen School: The Historiographical Issues” (2003).

4. On the importance of making assumptions as explicit as possible, see Long 2002, esp. 382–84.

5. With apologies to Childs, author of *Biblical Theology in Crisis* (1970).

6. In addition to Iggers, see Fox-Genovese and Lasch-Quinn (1999), and the essays in Spiegel 2005.

a consensus that is rejecting the excesses of post-modernism.⁷ For example, many scholars have accepted some of the basic criticisms of post-modern historians, and now speak of the intrinsic speculative element in history writing (Jordanova 2000, 111), minimizing bias, rather than eliminating it,⁸ and emphasizing a “best explanation” rather than certainty.⁹

To my mind, the most useful metaphor for understanding the nature of history is that of a map; this is developed quite fully in the recent *The Landscape of History: How Historians Map the Past* by Gaddis (Gaddis 2002). He emphasizes that just as maps represent places, historical writing represents the past, but cannot replicate it: “We avoid the literal in making maps because to do otherwise would not be to *represent* at all but rather to *replicate*. We’d find ourselves drowning in detail: the *distillation* that’s required from the comprehension and transmission of vicarious experience would be lost” (2002, 32). Gaddis acknowledges that “there is no such thing as a single correct map” (2002, 33), but does not give up on cartography or history writing. He says instead: “Cartographic verification is, therefore, entirely relative: it depends upon how well the mapmaker achieves a fit between the landscape that’s being mapped and the requirements of those for whom the map is being made,” and going on to say, “it would be imprudent for historians to decide, from the fact that they have no absolute basis for measuring time and space, that they can’t know anything about what happened within them” (2002, 34). Maps and historical reconstructions (or better, historical “representations”)¹⁰ are judged on their “usefulness” for particular purposes (2002, 46); thus Gaddis accepts post-modern critiques of the notion of an absolute recoverable truth, but finds a compelling way for historians to continue to perform their craft. In fact, such critiques might even have a positive influence on reconstructing history. The provocatively titled *Is History Fiction?* notes: “History wars, wherever they occur, have a way of driving historians back to the sources, checking the

7. See Fulbrook 2005, esp. 98–121.

8. See, for example, McCullagh 2000, 60: “To minimize the possibility of bias, historians should check that their descriptions, interpretations, and explanations are well supported by the data concerning their subjects that is available to them.”

9. McCullagh 2004, 21: “How do historians arrive at particular facts about the past? They normally interpret observable evidence, looking for the best explanation of it in the context of what they take to have been the circumstances of its origin.” McCullagh’s essay in its entirety is a polemic against postmodern approaches. See also the critique in Tucker 2004.

10. Gaddis 2002, 136: “history, like cartography, is necessarily a *representation* of reality.”

relationship between historical narration and analysis on the one hand and the documentary and other records on the other" (Curthoys and Docker 2005, 232).

Returning and reanalyzing the sources of the past is far from a straightforward venture. As Southgate notes:

[A]lthough common sense indicates that an actual past existed, it was never a simple entity that could be easily comprehended, perceived or understood... Any evidence deriving from that past is therefore inevitably partial—partial in the sense of being only a part (an infinitesimal part) of what it might be, and partial in the sense of being not impartial, not "objective." All that we can do with that evidence, therefore, is to evaluate it, make some provisional sense of it, and then use it to give an account that seems to us to have meaning. (Southgate 1997, 124–25)

There is a strong sense that creating good history requires a variety of evidence that can be compared (Tosh 2000, 65–66), and, following E. P. Thompson, that evidence must "be interrogated by minds trained in a discipline of attentive disbelief" (2000, 69).¹¹

The foundational importance of evidence to the historian is a theme of Vincent's *History*, which opens with the observation that "History is about evidence" (Vincent 2005, 9). It is not about the past, since "Studying the past is not possible: it is no longer there. All that can be studied are particular pieces of evidence..." (2005, 9). Using evidence, however, is quite complex because "evidence flagrantly distorts. There is a bias in the creation of evidence, and a bias in the survival of evidence." This problem means that all historical reconstructions are by nature imperfect: "No evidence, no history; imperfect evidence, imperfect history" (2005, 77). It also suggests that evidence must be used with great care, since "All evidence tells us something, is a symptom of something, is not to be brushed aside" (Vincent 2005, 22–23).

How this evidence is interpreted, however, is often contentious, and is never certain. As the American historian David Hackett Fischer said in his early book on historical method, "[A]ll inferences from empirical evidence are probabilistic. It is not, therefore, sufficient to determine merely that *A* was possibly the case. A historian must determine, as best he can, the probability of *A* in relation to the probability of alternatives" (Fischer 1970, 63). This means that claiming that an event is *possible* is of no value to the historian.

11. See also Morley 1999, 93: "Whatever you do, don't just believe everything you're told; every statement should be taken apart and scrutinized before, reluctantly, you accept that it might conceivably be true."

B. Biblical Historiography

Biblical scholarship has become more "normal" in the last two decades, by which I mean it has looser ties to theological studies, with their special rules, and closer ties to the general humanities and social sciences (Brettler 1995, 5). As such, it should not be surprising that many biblical scholars have been influenced recently by trends of general historiography, especially the warnings that it has offered about using evidence, and the discussions about how narrative elements in historiography may obscure the recreation of the past.¹²

Some biblical scholars, like their post-modern general counterparts, have moved toward extreme skepticism about the value of the Bible as evidence for the past it purports to narrate.¹³ Thomas Thompson, for example, calls the DtrH the "mother of all fictions" (Thompson 1994, 86). An opposing position has been argued recently by Kofoed, who claims that the Bible "must be *included in* rather than *excluded from* the pool of reliable data for a reconstruction of the origin and history of ancient Israel" (Kofoed 2005, 4–5) and that "the texts of the Hebrew Bible contain reliable information for a reconstruction of the period it purports to describe..." (2005, 30).

Most scholars, however, believe that both of these positions are tendentious, and should be disregarded. For example, in reference to recreating the period of Solomon, Miller notes:

Everything we know, or think we know, about the historical Solomon rests ultimately on the Hebrew Bible. Yet the biblical materials have a long and complex history of their own and the Solomon which they present is a largely legendary figure. I reject the short-cut approaches of Millard (brush aside the Bible's literary complexities and take its account of Solomon essentially at face value) and Thompson (dismiss the biblical account of Solomon as useless for the historian's purposes and declare Solomon unhistorical), and favor a middle course (attempt with the tools of literary-critical analysis to extract from the Bible at least some bits of authentic information about the historical Solomon). I am aware that the literary-critical analysis has its own problems and that this middle course is likely to produce very little in the way of undisputed conclusions. Yet I see this as our only option. (Miller 1997, 24)

I believe that this statement is equally true concerning the material in Samuel about Saul and David—there is no external evidence to suggest that we accept it or reject it. The question thus becomes: Can we use "the

12. See the discussion in Henige 2005.

13. This position is often associated with the Copenhagen School; see above, n. 3.

tools of literary-critical analysis to extract from the Bible at least some bits of authentic information” about the historical Saul? It may not be denied *a priori* that “there might be factual data [about Saul] preserved in the biblical text” (Edelman 1996, 142), nor should it be accepted *a priori* that “David and Solomon belong to the age of legendary beginnings rather than royal record” (Auld 1996, 167). However, as a result of a careful look at the sources, we might reach one of these opinions, or some position in between.

We must, however, use the sources in Samuel with great care, and not make any initial assumptions about their nature. Na’aman offers a warning to those who see much archival material and an extensive library upon which the authors of Samuel drew (Na’aman 1996, 180–83). He starts—and ends—in the middle, somewhat like Miller: “There is nothing impossible about the account of David’s conquests—the only problem is whether or not it really happened. Unfortunately, the sources for this episode are of such nature that we are unable to answer the question with a definite ‘yes’ or ‘no’” (1996, 183). As I would phrase it, each biblical account that contains evidence that may be useful for reconstructing the period it purports to describe should be treated as *potential* evidence for that period, and only once internal and external investigation of this source is completed may we determine its use for reconstructing history, or using Fischer’s observations above, the likelihood or probability that “A” is the better case relative to alternative scenarios. After the examination of biblical sources using the standard tools of general historiographical method,¹⁴ a source may end up being helpful, for example, if we determine it is written very close to the event by a reasonably unbiased observer; or it may be useless, if we determine that it was written a long time after the events it purports to describe, and is not based on sources; or we may be unsure, as Na’aman notes above, whether it is useful or not. Such judgments, however, can only be made after a careful consideration of the material in chs. 24 and 26. I will preface my treatment of these chapters with a survey of how they are dealt with in two very different genres of biblical scholarship: histories of Israel, and commentaries on Samuel.¹⁵

14. For this purpose, I have generally relied on Shafer 1969.

15. One surprising feature of the breakdown of scholarly work into these two genres is the extent to which they are truly separate, and rarely cross-reference each other. This is surprising, since both use the same source—Samuel—and I would have imagined that exegetes would have wanted to set their interpretation within historical settings, and historians would have depended on the close analyses performed by commentators. This is not, however, the case.

III. Histories of Israel

The late 1950s saw the publication of two major histories of Israel: that of Martin Noth, in 1958, and that of John Bright, in 1959.¹⁶ These are often seen as representing the culmination of scholarship through the mid-20th century in Germany and in the US, representing the schools of A. Alt and W. F. Albright, respectively. Thus, in terms of the history of scholarship, these two works are unusually important.

Noth takes for granted the existence of David and Saul, and is enthusiastic about the usefulness to the historian of the sources that deal with the conflict between Saul and David. He believes that the sources concerning “David’s rise to power” show “obvious expert knowledge and a sure grasp of the underlying circumstances” (Noth 1960, 179). Noth speaks of David’s escape from Saul, David’s assembling “together a band of adventurers,” and notes that David was “pursued all the more by the hostility of Saul, who failed, however, to catch him” (1960, 180). Although he paraphrases sections of the biblical narrative in his *History*, Noth does not paraphrase or specifically recount in detail the material found in 1 Sam 24 and 26. He does not spell out why he includes particular details, but excludes others concerning the transition from Saul to David.

In contrast, Bright begins his discussion by noting that the material from 1 Sam 1–1 Kgs 11 is “of the highest historical value, much of the material being contemporaneous, or nearly so, with the events described.” In terms of the source incorporating 1 Sam 24 and 26, he notes that these are “not historical records in any strict sense” but “were of ancient origin and in fixed form by the mid-tenth century.” He claims: “We are, in short, better informed about this period than any comparable one in Israel’s history” (Bright 1981, 184).

Given his starting point, it is not surprising that Bright paraphrases the biblical material, noting, for example, David’s “dodging continually to escape the clutches of Saul (chs.23:19 to 24:22; ch.26)” (Bright 1981, 193). Unlike Noth, he specifically cites chs. 24 and 26 as sources, though he does not clarify the extent to which they contain precise dependable information. Their reliability derives from the belief that the account of David’s succession is early Solomonic, and that it knows the traditions concerning Saul, so these traditions must be accurate (1981, 184).

16. I will be citing subsequent editions: Noth 1960 and Bright 1981—that is, the last edition that Bright lived to see.

The publication in 1986 of Miller and Hayes's *A History of Ancient Israel and Judah* was seen as a landmark event.¹⁷ It was published as a crisis began to loom in the writing of biblical histories. It followed by several years two books which questioned the common reconstructions of the patriarchal period (Thompson 1974; Van Seters 1975), and was written as there was a growing awareness of the literary features of the Bible,¹⁸ including the DtrH, which called into question the extent to which biblical historians were primarily concerned with the facticity of the events they were describing. It is thus not surprising that this work is much more methodologically self-conscious than its predecessors.

Miller and Hayes's discussion of the sources of Samuel is quite different from that found in Bright. They note that “[t]he various independent traditions that have been combined to produce I-II Samuel are folk legends for the most part” (Miller and Hayes 1986, 124), legends that “tend to glorify or condemn the personalities involved, emphasize the dramatic, include novelistic features such as conversations between private individuals, lack any clear chronological framework, and so forth.” Miller and Hayes note the clear ideological bias of this part of Samuel, where material has been edited and modified “to emphasize the legitimacy of the Davidic dynasty (rather than Saul's descendants whom David displaced)” (Miller and Hayes 1986, 125). In reference to what they call “*The Stories about David's Rise to Power*,” they observe: “Some of these stories seem contradictory and some seem to be duplications, but they all have one common theme—David's rise to power and his rightful claim to the throne” (Miller and Hayes 1986, 128–29). It is therefore not surprising that these authors note that the function of 1 Sam 26 is to “anticipate that David will replace Saul in this capacity [as king],” and they should not be used in a simplistic manner to recreate history (Miller and Hayes 1986, 155). In analyzing these stories in detail, they suggest that 1 Sam 23:19–29 and chs. 24 and 26 are

three versions of a single incident, in which case we regard the one in 1 Sam 23:19–29 as more convincing. The other two are more novelistic in their style, their scenarios as to how David spared Saul's life are hardly plausible, and their emphases on David's loyalty to Saul in his capacity as “Yahweh's anointed” sound too much like pro-Davidic propaganda. (Miller and Hayes 1986, 166)

17. The significance of this publication may be seen in Brettler 1987.

18. As a point of reference, Robert Alter's *The Art of Biblical Narrative* was published in 1981.

Miller and Hayes thus accept the chapters about David fleeing from Saul in their broadest outline, but find chs. 24 and 26 to be of little or no value in reconstructing the historical relationship between David and Saul, whom they consider to be historical figures. Other authors of recent biblical histories have expressed a similar position.¹⁹

The last few years have seen reactions in both directions to what might be called the “moderate” position of Miller and Hayes. In fact, 2003 saw the publication of two opposing, large-scale histories of Israel, each of which indicates its stance through its title: Liverani's 2005 *Israel's History and the History of Israel*, and Provan, Long, and Longman's 2003 *A Biblical History of Israel*.

Liverani, best known as a historian of the ancient near east, claims that his *History* is the first of this genre that does not paraphrase the Bible (Liverani 2005, xv). Yet in his reconstruction, he assumes the existence of David and Saul, and speaks of “The ‘Charismatic’ Kingdom of Saul” (Liverani 2005, 88). He recreates a brief history of Saul, noting that it was “used in different times and ways.” He speaks of a “denigration” of Saul in the text that “may have begun already in the time immediately following the events, in order to give more prestige to David” (2005, 91). Liverani's depiction of the history of the transition between Saul and David is more minimal than that found in Miller and Hayes, but is not fundamentally different from it—both histories are skeptical of the sources, though they accept the existence of the main personages mentioned (David, Saul), and some vague outline of the biblical text.

Provan et al. take a different approach concerning the sources of Samuel. They emphasize literary methods, which suggest to them that even if there are apparent inconsistencies, the Bible should be taken as a whole, rather than divided into sources (Provan, Long, and Longman 2003, 198). Based on the mention of David in the Tel Dan inscription and in (the reconstructed) Mesha inscription, they call the assumption that David existed “reasonable” (2003, 216–17). They object to several

19. See, for example, Meyers 1998, 230: “...the preponderance of nondocumentary passages makes them [i.e. accounts of Saul, David, and Solomon—but especially Saul] less useful to the historian than their historiographic cast and vivid prose suggest.” The perspective in Ahlström (1993, 423) is similar: “Because the biblical traditions about the rise of the monarchy of Saul (and then of David and Solomon) are late editions in their present form and not the result of any scribal activities of the royal courts of the eleventh century B.C.E., there is no possibility of assigning any particular tradition to this early part of the kingdom of Israel.” Later (p. 432) he suggests that some of the Saul material “is certainly not fiction” although it “has been shaped by particular Judahites who had to legitimate David's usurpation of the throne.”

recent treatments of David which have read the biblical stories “against the grain,” claiming that their “approach is to adopt a more robust confidence in the power of testimony to convey true information (including ‘spin’) about the past, unless and until sufficient contrary evidence emerges to undercut that confidence.” They go on to disprove, in their view, the major negative example concerning the David material—the smiting of Goliath by David (2003, 221–25), and suggest that if the David and Goliath story could be true, other material about David should be seen as true. Though they do not offer a detailed retelling of the history of Saul and David,²⁰ they believe that their “sounding in one stretch of text may encourage confidence in the rest, but it does not guarantee it” (2003, 227). In conclusion, they observe that the text has some ideological purposes, but nevertheless, “In the end we found little reason to question the value, for the historian, of the biblical testimony to Israel’s transition to kingship” (2003, 237).

How do these different approaches hold up against the general observations of Section IIA above, concerning the reconstruction of history from evidence? To my mind, they are all problematic. Most problematic from a methodological standpoint is the claim in Provan et al., whose approach, as noted, accepts the biblical account “unless and until sufficient contrary evidence emerges to undercut that confidence.” As I observed above, all evidence needs to be evaluated, and to be treated with suspicion—there is no general historiographical principle that suggests that *any text* should be taken as true until disproved.

The other histories are also problematic. None of them examines in sufficient depth the biblical sources about Saul. None offers *evidence* that the sources about Saul are reliable enough to assume that Saul exists. None looks at the biblical text from the assumption that the Saul narratives must contain evidence about something, but asks: Is it more likely that they contain evidence about a real king named Saul, or a constructed king named Saul? None of them adequately examines the history of composition of the HDR, the History of David’s Rise, in which chs. 24 and 26 are embedded—the date of the HDR,²¹ the extent to which it is a unified composition or has significant additions, and the extent to which its putative editor revised his earlier sources to create a more coherent document. Until these basic questions are probed in depth, offering different arguments for different possibilities and probabilities, it is silly to talk about evaluating the likelihood of specific events that the Bible narrates of Saul. After all, if the evidence does not even suggest that it is

20. See their justification of this on p. 237.

21. For some opinions on this, see Mettinger 1976, 41.

probable that Saul existed, it is silly to probe whether or not Saul chased David in the Wilderness or Ziph or Ein Gedi! In other words, from a general historiographical perspective, the treatments of 1 Sam 24 and 26 in the histories I have surveyed are inadequate and problematic.

IV. *Commentaries*

The commentary tradition on Samuel is very extensive, and it is not practical to review the it in its entirety. Rather, I will highlight major commentaries, and will attempt to show (1) the extent to which commentators treat the two episodes as doublets; (2) how they account for the similarities, in the event that they see the chapters as doublets; and (3) which chapter they see as earlier and why.

Smith’s ICC commentary of 1899 follows the position of Budde and some other German scholars (Budde 1902, 157),²² who saw the Pentateuchal sources continuing into the נבאים ראשנים, the prophetic historical books, and thus saw chs. 24 and 26 as JE doublets, though unlike Budde, Smith sees ch. 26 as J, and 24 as E (Smith 1899, 216, 229–30).²³ Although he offers no details, he suggests that there is a “slight preponderance of probability” (Halpern 2001, 216) that ch. 26 is earlier.

The decades separating Smith from Driver’s *Notes on the Hebrew Text and the Topography of the Books of Samuel* (Driver 1960) saw, by and large, the abandonment of documentary models from Pentateuchal studies being applied to Samuel (Clements 1976, 31–50). Driver, ever-cautious, notes that “the two [narratives about David fleeing from Saul] have been held by many scholars to be in reality different versions of the same incident. If this opinion be correct, the more “original” version is ch. 26. Quite significantly, he notes that the parallels extend beyond these two chapters, observing that 26:1 is “largely identical” with 23:19 (Driver 1960, 204). The focus of his book may explain the lack of detail in explaining why ch. 26 is likely later—he is primarily interested in “lower critical,” that is, textual issues, rather than “higher critical” or compositional issues.

Hertzberg’s 1956 ATD commentary was translated into English in 1964 as part of the OTL series (Hertzberg 1976). He notes that ch. 26 and 22:19–24:22 “make use of the same material” (1976, 207), and spends time explaining why the editor included both stories, rather than

22. This position is modified slightly in Dhorme 1910, 235.

23. A different documentary approach is taken in Halpern 2001, 278–79. He sees the traditions that underlie the depiction of David as a bandit as having “arisen no later than the 9th century” (p. 287).

exploring which is first (1976, 208). Although earlier German scholars had explored this issue,²⁴ it is unusual, even at this period, to focus on redactional issues without also looking at compositional ones.²⁵

Although McCarter, in his AB commentary (McCarter 1980), is very interested in textual issues, in part because of his access to many Dead Sea Scroll fragments before they were published, he does not ignore compositional issues. In his introduction, McCarter accepts the position of Rost, that 1 Sam 16:14–2 Sam 5:10 is a separate source, called “The History of David’s Rise” (McCarter 1980, 27–30).²⁶ McCarter notes the apologetic nature of this source, and rejecting the idea of several scholars that it developed early in the divided monarchy (1980, 28), claims that “the HDR is a document from the time of David himself” (1980, 29). Given the duplication between chs. 24 and 26, he suggests that only ch. 26 was part of the HDR, while ch. 24, or actually 23:14–24:23, is “a later tendentiously fashioned equivalent to 26:1–25,” which nevertheless preserves some traditional materials (1980, 387). The arguments adduced by McCarter are more detailed than previous scholars.²⁷

By the late 20th century, it was impossible to ignore the evidence that there was a clear connection between the two stories related to Saul chasing David. The evidence, however, might be interpreted differently. For example, Gordon suggests: “The points of similarity between chs. 24 and 26 have led many scholars to think of them as sibling versions of the same original episode. But there are many basic differences between the two, and it is probably easier to account for the similarities by assuming a degree of assimilation in their early transmission history” (Gordon 1986).²⁸ Other commentaries note the connection between these units, but seem to have tired of dealing with issues of genetic connection.²⁹ This is especially true in works which stand somewhere between being commentaries and literary readings.³⁰

24. See Gressmann and others, cited in Hertzberg 1976.

25. In more recent studies, however, this has become commonplace; see, among others, Gordon 1980, 36–64; Jobling 1998, 92; and Harvey 2004, 61–65.

26. The fundamental work of Rost has been translated as Rost 1982.

27. They are, however, not as compelling and methodologically sophisticated as those found in Edenburg 1998, 64–85.

28. A similar position is found in Caquot and de Robert 1994, 318.

29. See, for example, Ackroyd 1971, 188, 202. Campbell (2003, 252) states: “There is little point in looking for a relationship of dependence between the two tellings. It is enough that there are two tellings of one tradition, each quite capable of standing on its own.”

30. See, for example, Gunn 1980, who does not treat the connections between chs. 24 and 26, and Garsiel 1983, 134–45, who sees chs. 24–26 as a single unit, though he speaks of 24 and 26 as “parallel” and “double” stories, but does not

In sum, most critical commentaries see the two chapters as doublets, though modern literary scholars prefer to find alternative explanations for the similarities. Among those surveyed above, the scholars who see the chapters as genetically connected by and large follow Wellhausen’s position³¹ that that ch. 26 is earlier,³² with one scholar suggesting that the similarities should be explained by assimilation of elements between the chapters. Some other scholars consider the possibility that the two episodes are best treated as oral variations (Grønbæk 1971).³³ Koch suggests that “On the whole chapter xxvi (B) appears to be a later stage of the tale” (Koch 1969, 143). There is some disagreement about whether the two chapters alone should be studied as parallel, or if the parallelism extends back into ch. 23 as well, and if so, how extensively that chapter should be considered. There is significant debate on how these chapters should be dated. Given this lack of consensus in a variety of areas, I will examine these episodes in detail, seeing the extent to which compelling evidence may be adduced, so that we may begin to understand the David Tradition and its development.

V. A Close Analysis of 1 Samuel 24 and 26

As with “normal” historical texts, 1 Sam 24 and 26 must be subject to close analysis before they are used by the modern historian interested in reconstructing the history of Israel—both external and internal analysis must be completed (see, e.g., Shafer 1969, 99–142). After completing these analyses, it is possible to see in what way(s) these texts may be used for evidence for the period of Saul or for some other period, since after all, as noted above, “All evidence tells us something, is a symptom of something.”³⁴

analyze them as such. Miscall (1986, 144–62) also treats chs. 24–26 as a unit, calling ch. 26 “A Replay of Power Play” (p. 158), but shows no interest in examining how the two are related from a compositional perspective. Polzin (1989, 205–15) begins by critiquing those who look at chs. 24 and 26 as genetically related parallels, and also treats chs. 24–26 as a unit. Fokkelman (1986, 531 and *passim*) shows awareness of some parallels between chs. 24 and 26, but has no interest in explaining these in other than literary means. Bar-Efrat (1996, 313) takes a similar perspective.

31. See above, p. 25.

32. Segal (1971, 188) also agrees with this consensus.

33. Culley (1976, 53–54) reaches no conclusion about whether the similarity is from copying or from oral tradition similarities. McKenzie (2000, 95) is agnostic, saying: “It is not clear whether one was written on the basis of the other or each is a separate version of a common tradition.”

34. See p. 28, above.

A. External Sources Bearing on These Texts

There is no external evidence that bears on any aspect of the historicity of 1 Sam 24 and 26. I will leave for others to decide if there is *any* archaeological evidence that bears on David and the Davidic kingdom. But for this context it is sufficient to note that there is no archaeological evidence that bears on Saul's persecution of David before the latter became king. I will go further, and state that I need to be careful in stating that David son of Jesse became king over a portion of Israel. There too, we have no contemporaneous evidence. I believe that the much discussed Tel Dan inscription is authentic,³⁵ and most likely does refer, in the expression בֶּן־דָּוִיד, to a king named David (rather than a place name), but this has no bearing on the existence of a real David, King of Judah, in the 10th century. All scholars agree that the inscription postdates the putative period of David by at least a century,³⁶ and thus it proves no more than the fact that the inscription's author believed in a Davidic dynasty.³⁷ Thus, using normal historical methods, I leave open the question of the existence of David when I discuss these issues, assuming neither that he had to have existed, nor that that he definitely did not exist. One of the questions I will have to examine is whether or not the internal evidence bears on the existence of David.

B. Internal Analysis

Internal criticism involves a large number of questions, not all of which are relevant to these texts. They include examining the distance between the putative event and its telling, authorial intention, and internal contradictions.³⁸ After asking these and other questions, the historian must ask: "Does the statement leave you sufficiently confident of your knowledge of that detail so that no corroboration is required?" (Shafer 1969, 138). I will look at these various questions, beginning with the issue of internal contradictions.

35. To the best of my knowledge, there are witnesses who saw it recovered *in situ*, and there are no compelling physical arguments that it is a forgery.

36. For the inscription, and bibliography on its date, see *COS II*, 161–62, and more recently Athas 2003.

37. Note Athas 2003, 309: "[W]e cannot say that we have pinned David down outside of the pages of the Bible... The Tel Dan Inscription does not give us proof of an historical David, but it may certainly be admitted as evidence." Contrast the approach of Arnold (2005, 871), who claims that the Tel Dan inscription and archaeological evidence "point to David's existence." A more moderate view concerning the use of external evidence is found in the same volume, in Satterthwaite 2005, 204–5. See also *COS II*, 162 n. 11.

38. Shafer 1969, 121–42, esp. the checklist on 187–88.

Chapters 24 and 26 are contradictory. It is not only the case that they tell the same story differently, but their repetition suggests a number of logical problems. As noted by Koch: Why would David flee to Ziph a second time, after recognizing that the Ziphites are allied with Saul? Why would David's men again suggest that he kill Saul, after David rebuked them strongly earlier? And why does ch. 26 show no awareness of what happened in ch. 24, using, for example, the word טוֹב?³⁹ This suggests that one or both of these episodes is secondary—in other words, either ch. 24 is based on ch. 26, ch. 26 is based on ch. 24, or both ch. 24 and ch. 26 are based on a lost source.⁴⁰ It is important to try to resolve this issue, since if our goal is to reconstruct events surrounding David and Saul, primary evidence is of more value than secondary evidence. Evidence dependent on another source would have to be considered secondary, and thus must be isolated.⁴¹

Determining when a text is quoting another text, the directions of borrowing, and how to reconstruct the *Urtext* are common problems in the study of religious literature. Although many scholars have explored this issue in recent decades, there is no consensus about the criteria—this is why, for example, there is no single theory explaining the development of the Synoptic Gospels, and no universally recognized reconstruction of Q (Tuckett 1992). Many recent studies have focused on echoes and allusions—in other words, what criteria might be used for determining that a later scholar used, and is intentionally referring back to, a well-known, earlier work. This issue has been explored by Hays in reference to the Pauline letters, where he developed seven criteria for establishing echoes: availability, recurrence, volume, thematic coherence, historical plausibility, history of interpretation, and satisfaction (Hays 1989). Sommer has investigated this in detail in Isa 40–66. He is especially careful to define his terms, and it is likely that he would consider the case of 1 Sam 24 and 26 as a potential case of "revision," where "[a]t times a biblical text will restate some aspects of an earlier text while altering elements of the older text's message or adding to the earlier message" (Sommer 1998, 25).

39. Koch 1969, 142; I have modified his three points slightly.

40. I am using literary rather than oral models here; even Culley, who helped to develop ideas of how understanding oral literature might help understand doublets, was not convinced that the differences between these two chapters can be reasonably accounted for on the oral level; see Culley 1976, 49–54.

41. There is some debate about defining primary vs. secondary evidence; see Kofoed 2005, 41–42; Tosh 2000, 38; and Jordanova 2000, 101.

In an important theoretical examination concerning “Uncovering Literary Dependencies in the Talmudic Corpus,” Friedman outlines two tendencies among rabbinic scholars who study synoptic passages (Friedman 2000, 35–57). One is to explain similarities as “independent parallels,” where there is no direct literary dependence of one text on the other (2000, 36–38),⁴² while the other is the “edited parallel” (2000, 38–39). From his experience with rabbinic texts, Friedman favors the latter model, and I believe his arguments are valid for studying 1 Sam 24 and 26 in relation to each other as well.

Unfortunately, Friedman does not offer general principles for determining which text is more original. Using some of the insights of Hays, Sommer, and Friedman, I will try to do this below.

Two main factors suggest that 1 Sam 24 and 26 are related: they both have the same structure, and they share a significant amount of vocabulary. These criteria need further refinement. Not all cases of shared structure and similar storyline are clearly related—in some cases, the similar or identical structures may be the result of generic constraints (e.g. many ancient letters start the same way), or the result of narrative considerations (e.g. many people’s flood stories end with the boat landing on a mountain—but where else should it land?). Furthermore, structural links need to be specific—a few general similarities between two stories are of little significance. Finally, in terms of claiming that two texts are genetically related, the vocabulary links need to be unusual, and (within reason) in the same order—a few scattered shared common words are not sufficient.

These two chapters do not partake in a general genre that would have informed their shaping. It is difficult to believe that there was a genre of “future kings being chased by current kings, where the future king had the opportunity to kill the king, but did not because he was YHWH’s anointed.” This is not the sort of broad plot or theme that would give rise to clearly delineated structures. In fact, there are only two detailed stories with this plot—1 Sam 24 and 26—and it would be hard to claim that a genre existed on the basis of these two stories alone. Similarly, although there are some sub-motifs that the two chapters share because they are a natural part of the story—for example, both begin with the king being told where the future king is (24:1–2; 26:1), and both end with a parting of ways (24:23b; 26:25b)—there are many unexpected elements that both stories share, such as the future king’s officers wanting to kill the king (24:5a; 26:8).

42. This is similar to Culley’s work on oral literature.

The criteria just adduced are not phrased in purely objective terms. I did not say how precisely the structure must match, or how many terms must match, and how many of these need to be rare, and what qualifies as rare. I do not believe that it is possible to quantify these features. But I do believe that the evidence that I will adduce below concerning chs. 24 and 26 shows substantial similarities in structure and terminology, and is sufficient.

The two chapters may be outlined as follows:⁴³

<i>Structural Element</i>	<i>Chapter 24</i>	<i>Chapter 26</i>
Saul told where David is	1–2	1
Saul goes to that place	3	2
Action that would allow David to harm Saul	4	3–7
David’s officer(s) want David to kill Saul	5a	8
David refuses to kill Saul	6–8	9–11
David performs alternate action	5b	12
David calls to Saul(’s party)	9–16	13–16
Saul responds	17–22	17a
David responds	23a	17b–20
Saul responds		21
David responds		22–24
Saul responds		25a
Parting of ways	23b	25b

As this outline indicates, the two stories are structurally very similar. The main differences are that ch. 26 has more back and forth dialogue between David and Saul, and there is a difference in the narrative chronology concerning David refusing to kill Saul, and performing an alternate action: in ch. 24, David first cuts off the corner of Saul’s robe (v. 5b), and then says that he will not killed YHWH’s anointed, while in ch. 26 he first says that he will not kill YHWH’s anointed (vv. 10–11), and then steals Saul’s spear and water jar (v. 12). Especially once the evidence of the shared vocabulary is adduced, the commonalities between these two units are significant enough to suggest that they are very likely dependent variants, one of the other, or are based on a common earlier story.

The shared vocabulary and phrases are listed below; it is followed by an analysis of how frequent the items are:

43. For similar outlines, see Koch 1967, 133–37; Green 2003, 374–75; Edenburg 1998, 49–53; Halpern 1988, 63–65.

<i>Chapter 24</i>	
3	שְׁלֹשֶׁת אֲלָפִים אִישׁ בַּחוֹר מִכְלִי־שָׂרָאֵל
3	לְבַקֵּשׁ אֶת־דָּוד
4	וְדוֹד וְאֱנֶשׂיו בִּירְכְּתִי הַמְּעָרָה יְשִׁבָּת
5	הָוּם... אֶת־אַיִבֵּךְ [אַיִבֵּךְ] בִּידֵךְ
7	הַלִּילָה לְיִמְחֹה
7	לְשָׁלֹחַ יְדֵיכְ בְּכִימְשִׁיחַ יְהֹוָה הוּא
11	בִּמְשִׁיחַ יְהֹוָה
15	יֵצֵא מֶלֶךְ שָׂרָאֵל
15	פְּרֻשׁ אֶחָד
17	הַקְּלָךְ וְהַבְּנִי דָּוד
19	סְגָרִין יְהֹוָה בִּידֵךְ

<i>Chapter 26</i>	
2	שְׁלֹשֶׁת־אֲלָפִים אִישׁ בַּחוֹר יִשְׂרָאֵל
2	לְבַקֵּשׁ אֶת־דָּוד
3	וְדוֹד יְשִׁבָּת
8	הַיּוֹם אֶת־אַיִבֵּךְ בִּידֵךְ
11	הַלִּילָה לְיִמְחֹה
9	שְׁלֹחַ יְדֵיכְ בְּמִשְׁיחַ יְהֹוָה
16	עַל־מִשְׁיחַ יְהֹוָה
20	יֵצֵא מֶלֶךְ יִשְׂרָאֵל
15	פְּרֻשׁ אֶחָד
17	הַקְּלָךְ וְהַבְּנִי דָּוד
8	סְגָרִין יְהֹוָה בִּידֵךְ

The number **שְׁלֹשֶׁת־אֲלָפִים** is found 49 times in the Bible, frequently referring to a military contingent (e.g. Josh 8:3; Judg 15:11; 1 Sam 11:8). However, with the passive participle of **בַּחוֹר** is found in these two texts only, followed by a personal name other than YHWH, is found only in these two contexts and 2 Sam 5:17 (= 1 Chr 14:8). The type of parenthetical clause introduced by **וְדוֹד וְאֱנֶשׂיו בִּירְכְּתִי** is unremarkable, found in several dozen cases. The words **אַיִבֵּךְ** and **הַיּוֹם** are only found together in these two verses. The formula⁴⁴ **חַלְילָה לְיִמְחֹה**⁴⁵ is found only in these two contexts and in 1 Kgs 21:3. Surprisingly, the phrase **מְשִׁיחַ יְהֹוָה** is found only eleven times in the Bible; even more surprisingly, seven of these are in chs. 24 and 26. Furthermore, **מְשִׁיחַ יְהֹוָה** is only used with the idiom **שְׁלֹחַ יְדֵיכְ** in these two chapters and in 2 Sam 1:14, which is likely related.⁴⁶ **יֵצֵא מֶלֶךְ יִשְׂרָאֵל** is used only twice in the Bible, in these two chapters. Outside of its use as a personal name, **פְּרֻשׁ** is mentioned in these two texts only. Likewise, the expression for “is this your voice” is found only in these two verses.⁴⁷ Finally the idiom **(בִּיד) פִּלֵּךְ** is only found in these two contexts and in 1 Sam 17:46. Such commonalities in vocabulary would be unexpected in unrelated texts, and it is thus likely that 1 Sam 24 and 26 are related. Such strong similarities go beyond “a degree of assimilation in their early transmission history” posited by Gordon.⁴⁸

44. On this formula, see JM, §166k.

45. Specifically, see the use of the verb **שְׁחַת** **יְהֹוָה** with **שְׁחַת** **יְהֹוָה** only in 1 Sam 26:9 and 2 Sam 1:14.

46. The importance of these two “unique recurrences” is emphasized by Edenburg 1998, 76.

47. See above, p. 36.

The very strong verbal and structural similarities suggest that the two chapters are related genetically,⁴⁸ but they do not tell us how they are related. Is ch. 24 dependent on ch. 26, is ch. 26 dependent on ch. 24, or are they both dependent on a common (oral or written source)? As we saw, a majority of scholars suggest that ch. 26 is more original than ch. 24, but is their evidence compelling? Before examining these two issues in detail, it is important to determine first the exact boundaries of the texts being compared, and whether or not additional texts should be added to the this comparison.

A piece of evidence adduced by Driver and others (see Driver 1960, 204) helps to fine-tune the exact boundaries of the material that is related. First Samuel 26:1 opens with an account of the Ziphites telling Saul where David is hiding:

וַיָּבֹאוּ הַזִּפִּים אֶל־שָׁאול הַגְּבוּעָה לְאמֹר הַלֹּא דָוד מִסְתָּחָר
בְּגַבְעַת הַחֲכִילָה עַל פְּנֵי הַשִּׁיםִין :

This is very closely parallel to 23:19:

וַיָּעֹלוּ זִפִּים אֶל־שָׁאול הַגְּבוּעָה לְאמֹר הַלֹּא דָוד מִסְתָּחָר עַמְנוּ בְּמִצְדֹּות
בְּחֶרְשָׁה בְּגַבְעַת הַחֲכִילָה אֲשֶׁר מִמֵּין הַשִּׁיםִין :

The strong similarities are noted below with underlining:

1 Sam 23:19
וַיָּבֹאוּ הַזִּפִּים אֶל־שָׁאול הַגְּבוּעָה לְאמֹר הַלֹּא דָוד מִסְתָּחָר עַמְנוּ בְּמִצְדֹּות
עַל פְּנֵי הַשִּׁיםִין :

1 Sam 26:1
וַיָּעֹלוּ זִפִּים אֶל־שָׁאול הַגְּבוּעָה לְאמֹר הַלֹּא דָוד מִסְתָּחָר עַמְנוּ בְּמִצְדֹּות
בְּחֶרְשָׁה בְּגַבְעַת הַחֲכִילָה אֲשֶׁר מִמֵּין הַשִּׁיםִין :

It is odd, however, given the number of parallels between chs. 24 and 26, that there are no significant parallels between 23:20–28 and ch. 26. McCarter notes that much of this intervening material is different from what follows, and is likely etiological; this suggests to McCarter that it is either a late addition, or an early part of the original story. Linguistic evidence indicates that it is most likely a late addition. As Driver already noted, the syntax of v. 20 is typical of “late Hebrew” (Driver 1960, 188). The syntax of v. 26, namely, followed by a personal name, followed by a Niphal, is only found in 1 Chr 4:9. The unexpected unconverted perfect in v. 27, PN **וּמְלָאֵךְ בָּאָלָל**, finds a parallel only in Job 1:14, which is Late Biblical Hebrew (Hurwitz 1974). This evidence suggests that 1 Sam 23:20–28 is likely a

48. In the words of Koch (1969, 53), “In these two narratives, the similarities are so striking that some kind of relationship has to be assumed.”

late addition, and our comparison should be between 23:19 + ch. 24 to ch. 26, rather than between 23:19–24:23 to ch. 26.⁴⁹ For convenience, I will use the shorthand of “ch. 24” to refer to 23:19 + ch. 24.

Halpern has suggested that both chs. 24 and 26 are based on the story in the appendix in 2 Sam 23:14–16 about David offering up some water that three of his warriors obtained for him.⁵⁰ Though there are some similarities between these stories, they are very general, and there is little overlap in language use between them. For example, the unusual phrase for the water jug found in ch. 26, *צפחת המים*, is lacking in 2 Sam 23. In general, Halpern is over-inclined to see biblical texts as reading and misreading each other (see Brettler 1990, 83–84), and it is thus best to see only 1 Sam 23:19 + ch. 24 and ch. 26 as the basis for analysis.

As noted in Section III, with few exceptions, most commentators bring only vague evidence concerning the direction of borrowing between chs. 24 and 26. The most systematic and compelling treatment of the connections is by Edenburg, who deals with both general methodological issues concerning similar texts, and the relation between these two particular texts (Edenburg 1998, 64–85). She establishes “five categories of evidence for establishing author devised interrelations”: “Unique recurrence of peculiar formulations,” “Similarity of content and/or structure,” “transformation and reactualization of a common element,” “‘Ungrammatical’ actualization of a common element,” and “Interaction between texts” (1998, 71–73). We have already seen that chs. 24 and 26 fulfill the first two criteria. Edenburg shows how they fulfill the fourth, “‘Ungrammatical’ actualization of a common element,” as well. By “Ungrammatical” she means that elements do not make sense in a particular text, but make sense in the other text, suggesting not only that the two are related, but that the element has been borrowed from the earlier to the later text—as she explains it: “An element in one text motivates the shape, formulation or topic of the other text” (Edenburg 1998, 73). She finds two odd elements in ch. 24 which are best explained through their dependence on ch. 26.

49. Several scholars (e.g. Wellhausen 1973, 264; Koch 1969, 137; Culley 1976, 49–54) claim that the unit that parallels ch. 26 begins already in 23:14, but given the strong parallels between 23:24 and 26:1, this is unlikely. In addition, the material in 23:14–18, though it shares some similarities with chs. 24 and 26, is much more general in outlook.

50. Halpern 1988, 62–65; note esp. 62, where he claims that chs. 24 and 26 are a “doublet” “constructed around the sort of exploit enshrined in 2 Samuel 23:14–17.” Edenburg (1998, 81) concurs.

She notes, correctly to my mind, that 24:7, where David says to his men *ויאמרו אנשי דוד אליו הנה נון אובי נון אהראיביך [איבך]* [בידך ועשה לו כאשר יתב בעיניך]. However, if ch. 24 is copying from 26:11 as it is revising it, this displacement is logically explained (Edenburg 1998, 76). Furthermore, Saul’s question *הקלך זה בני דוד* makes good sense in ch. 26, which is transpiring at night, but is very odd in 24:17, since that chapter is transpiring in daylight (1998, 77). She thus concludes that either ch. 24 is later than ch. 26, and the author of ch. 24 knew ch. 26, or that both chapters are by the same author, and ch. 24 is patterned after ch. 26. Ultimately, she favors the latter option (1998, 77–81).

Although her evidence is correct, I am not certain that Edenburg’s conclusions are correct. First of all, in terms of vocabulary, it is difficult to believe that these two stories are by the same author, so her idea that ch. 26 is “like a first born twin conceived by the same parent,” with ch. 26 as the first-born twin, and 24 born later, is unlikely.⁵² For example, why did ch. 24 not pick up on ch. 26’s unusual use of the verb *שחח* in the Hiphil in reference to killing Saul, using only *שליח יד*?⁵³ And where else in the Bible do we have a model of an author revising a work by composing a parallel to what he had already written, and inserting it near, but not adjacent to, the original chapter? The model of chs. 24 and 26 as written by two different authors makes more sense.

Chapter 24 is more integrated into the Samuel narratives than ch. 26. For example, the phrases *דוד ואנשי דוד* and *אנשי דוד*, found in ch. 24 and elsewhere in Samuel, are missing in ch. 26. In addition, it is odd that Abner is introduced as if unknown in 26:5, (“*וְאַבָּנֶר בֶּן־נֵר־צָבָא*” (*and Abner son of Ner was his [Saul’s] army commander*”), when he is known previously from 14:50; 17:55, 57; 20:25. In other more minor ways, ch. 24, but not ch. 26 is well-integrated into Samuel.⁵⁴ These pieces of

51. For other solutions to this issue, see Culley 1976, 51 and Gordon 1990, 139–44, esp. 142.

52. Edenburg 1998, 81; see the entire discussion on 78–81. I would go beyond the claim of Koch (1969, 144), “Nevertheless it is easy to see that A and B could scarcely have been taken out of the oral tradition and written down by the same writer at the same time” to claim that it is even unlikely that they were written by the same author at different times.

53. *שליח יד* is used in 26:9, 15; is found in 24:7, 11; 26:9, 11, 23.

54. The following correspondences are noteworthy: 1 Sam 24:2 and 1 Sam 14:46 (*מְאֹחֶרֶת פְּלִשְׁתִּים*); 1 Sam 24:5 and 1 Sam 18:22 (*בְּלִטָּה*); 1 Sam 24:8 and 1 Sam 22:13 (*קָומָם אֶל*)—the only occurrences of this term, which are connected thematically); 1 Sam 24:12 and 1 Sam 15:27–28 (tearing the corner of a garment—note especially

evidence raise a seeming paradox: based on Edenburg's findings, ch. 24 knows ch. 26, and is dependent upon it, but it is the later ch. 24 rather than the earlier ch. 26 which is better integrated into the book of Samuel! How can this be?

Having assumed that ch. 24 is secondary, Edenburg and other scholars have not examined ch. 26 closely enough to see if it is, to use Edenburg's term, "ungrammatical." I believe that it is, in at least one place, in v. 11–12a, which begins with David speaking to Abishai:

11 חָלִילָה לִי מִיּוֹהַ מֶשְׁלֵחַ יְדִי בְּמַשְׁיחַ יְהוָה וְעַתָּה קְחָנָא אֲזַהֲחָנִיתָ
אֲשֶׁר מַרְאָשָׁתוֹ [מַרְאָשָׁתוֹ] וְאֲזַכְּפָהָתָה הַמִּים וְנִלְכָהָ לָנוּ :
12 וַיַּחַד דָּוד אֲזַהֲחָנִיתָ וְאֲזַכְּפָהָתָה הַמִּים מַרְאָשָׁתוֹ שָׁאָל וַיְלַבֵּוּ לָהֶם

In v. 11, David says קְחָנָא to Abishai, but David himself takes the spear and water jug. Most likely this ungrammaticality indicates that ch. 26 is not pristine, but that it too is based on an earlier source, that it has reworked partially.⁵⁵ This evidence as a whole suggests that ch. 26 is based on a lost source, that ch. 24 is based on ch. 26, and that ch. 24 entered into the book of Samuel earlier than ch. 26, which is why it is thus better integrated into it.⁵⁶

If this reconstruction of the genesis of these two chapters, and the history of their placement in Samuel is correct, it is clear that ch. 26 is the better source for the historian trying to reconstruct the history of Saul and David—it is closer than ch. 24 to the putative events it describes. Chapter 24 knew ch. 26 more or less in the form we have it, and has no independent value as a historical source concerning David and Saul. But a "better source" is not always sufficiently good.

Chapter 26 is also problematic. If I am correct, it is based on a lost source, and we have no way of knowing how extensively it revised this source. This suggests, to return to one of the criteria mentioned above, that there is significant "distance between the putative event and its telling." This is bolstered by the language used in ch. 26—it is identical

the vocabulary links based on קָרְעַ, בְּנֵף מַעַיל (קָרְעַ, בְּנֵף מַעַיל); 1 Sam 24:21 and 1 Sam 13:14 (קָרְעַ + מַלְכָה); 1 Sam 24:21 and 1 Sam 23:17 (מַלְךָ + יָדָע).

55. The ungrammaticality was already noticed by Kimchi, who solved it in a manner that modern critical scholars are unlikely to find satisfactory, by saying (commentary to v. 12): "David took." After he said to Abishai 'Just take,' he changed his mind, because he did not want Abishai to approach him, for he thought that he would not be able to control himself and he would smite him; that is why David himself went and took the spear and water jug."

56. At a secondary level, ch. 26 is integrated into the broader narrative through the reference to the problematic נִבּוֹן—see 1 Sam 23:23 and 26:4.

to the language of Samuel–Kings, and I know of no evidence suggesting that this is in the Hebrew of the Davidic period.⁵⁷ Certainly, it is possible to claim that the text was updated linguistically, while its content was not changed in the process. However, that possibility is unlikely, and the evidence of Chronicles, which both updates the language of its source and updates and corrects it from a historiographical perspective,⁵⁸ is an instructive counterexample.

Internal analysis also suggests that we must examine, as we best can, the "author's intention." Here there is little debate within biblical scholarship about the function of the HDR, and of chapters such as ch. 24 and 26—they are apologetic texts that play a key role in legitimating David as Saul's successor (Brettler 1995, 91–111). As McKenzie puts it: "It goes without saying, in light of their apologetic nature, that the value of these two chapters for historical reconstruction is virtually nil" (McKenzie 2000, 96). I cannot agree with the logic of Satterthwaite, who claims that "The depth, complexity and (at times) ambivalence of these portrayals of David suggest that they are rooted in historical facts" (Satterthwaite 2005, 205). Such literary or rhetorical features have no bearing on the historicity of texts—if they did, we would have to consider many of the best mystery novels to be "rooted in historical facts."

As noted at the beginning of this long presentation regarding evaluating internal evidence, the crucial question that must be answered after performing the painstaking analysis is: "Does the statement leave you sufficiently confident of your knowledge of that detail so that no corroboration is required?" I believe that the evidence mustered above, which indicates that the earlier of the sources, ch. 26, is itself removed from the events it purports to narrate, and is highly ideological in nature, suggests that contrary to several of the histories of Israel mentioned in Section II, it is *not* prudent to use any part of these chapters in any way to reconstruct a real set of events where David is fleeing from Saul.

C. 1 Samuel 24 and 26 and the Historian of Ancient Israel

There are several loose ends that need to be tied up. I had earlier noted that "All evidence tells us something, is a symptom of something," so if this twice-told story does not reflect the period of Saul, of what is it a

57. The evidence concerning the linguistic dating of Hebrew has become very complicated in the last few years; see Young 2003; Zevit 2005; Kofoed 2005, 113–63; Fassberg and Hurvitz 2006.

58. For this purpose, it does not matter if we accept the traditional model that has Genesis–Kings as the main source of Chronicles, or A. G. Auld's model, developed in Auld 1994.

symptom? In addition, the conclusion I have reached contrasts sharply with that of all the historians in Section II. What may have misled these scholars, some of whom are quite comfortable elsewhere writing a history that deviates significantly from the biblical account?

The goal of authors of history should be “to provide a hypothetical description and analysis of some past events as the best explanation of present evidence. This knowledge is probably true, but it cannot be true in an absolute sense. The most that historiography can aspire for is increasing plausibility, never absolute truth” (Tucker 2004, 258). As I noted above, the most satisfactory or plausible explanation of this text is to explain the transition between Saul and David, to make David the Judean into a legitimate successor of Saul the Benjaminite. If this is correct, the text is a symptom of anxiety about the legitimacy of David as Saul’s successor. Such anxiety is possible even if Saul and David never existed as historical figures—it just needs to have arisen after the idea of David’s succession of Saul developed.⁵⁹ Though some scholars try to date the period when such anxiety was most likely present, and thus the period when the text was most likely written,⁶⁰ I do not believe that this is prudent. Thanks to the recent work of the scholars in the European Biblical Seminar and others, we now have a much better idea of how complicated it is to reconstruct the real history of Israel, and how little we really know. I doubt that we know enough about the ideological battles around Davidic kingship, who fought these and when, to justify a single most-likely date for the type of story found in chs. 24 and 26. Thus, given the very limited information we have about ancient Israel, and remembering the haphazard nature of this information, because “There is a bias in the creation of evidence, and a bias in the survival of evidence,”⁶¹ it is not prudent to guess who created these traditions, when, and thus how removed they are chronologically and ideologically from the events they purport to describe.

Why is the reconstruction in the histories of Israel so very different than what I have proposed here, both in terms of method and results? Some histories privilege biblical texts, and do not believe that the reconstruction of biblical history is bound by the same sorts of habits as the reconstruction of general history, including “attentive disbelief” of the source material.⁶² I believe that this is especially the case with

59. In terms of polemics and apologetics using David and Saul, Amit (2006) is very enlightening.

60. See n. 21, above.

61. See above, p. 28.

62. Recall the comment of E. P. Thompson, cited on p. 28 above, that evidence must “be interrogated by minds trained in a discipline of attentive disbelief.”

traditions concerning David, whose existence is very important for theological reasons for many scholars. Also, some scholars have compensated for the skeptical attitude toward many biblical traditions found in late 19th- and early 20th-century German scholarship by swinging the pendulum in the other direction.⁶³

Perhaps some believe that doubting the veracity of the biblical account is a form of Bible-bashing, but this is not true. As Fulbrook notes, texts that depict a past have many purposes, from teaching “information and understanding to empathy and emotive arousal” (Fulbrook 2005, 145). We are not making value-judgments when we claim that a text narrating *a* past is not narrating *the* past—we are just clarifying in what sense that text might be used as evidence. We would do well to remember the analogy that Coggins adduced between the Bible and Hamlet over twenty-five years ago: “We should laugh out of court anyone who approached *Hamlet*, primarily with a view of improving his knowledge of Danish history, or *Henry V* as a source of knowledge of 15th-century England; yet a very similar approach to many an Old Testament book is regarded as entirely natural and proper” (Coggins 1979, 43). There is nothing wrong in saying “The Israel of the literary tradition is not the Israel of history” (Kratz 2005, 309).

I hope that this essay will provoke discussion concerning the biblical sources, and how other pieces of the David tradition(s) might be used by historians. I strongly suspect that a more comprehensive look at these traditions would indicate that none of them may be used to reconstruct the history of Saul and David. I doubt that all will agree with this judgment, and will form a consensus on if and how the historical David should be reconstructed.⁶⁴ Many see this lack of consensus as a problem, and pine for the good old days, when we “knew” the history of Israel. Such nostalgic longing is misplaced. As Tosh has noted, it is good for history to be contested: “Plurality of historical interpretation is an essential—if underestimated—prerequisite for a mature democratic politics. The past will never be placed beyond controversy, nor should it be” (Tosh 2000, 133).

63. Concerning the traditions in 1 Sam 24 and 26, see Koch (1969, 140), noting that Caspari’s 1924 commentary claimed: “It would have been as easy to find the historical Saul as it is now to find a busy doctor at home.” It is striking that Koch, who quotes Caspari, notes (144): “it is possible here to arrive at a historical core. There is no doubt that Saul the king pursued his successful army leader, David. The hiding place in the wilderness of Ziph is certainly also historical...”

64. See the survey of depictions in Bosworth 2006.

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THE BEGINNINGS OF THE KINGDOM OF JUDAH

Philip R. Davies

In 1995, Avraham Biran and Joseph Naveh published the text of a stone monument from Tel Dan. Written in Aramaic to commemorate the capture of the city of Dan by a king of Aram from the kingdom of Israel, it contains the word *bytdwd*. Despite some dissenting voices, and arguments that the inscription might be a forgery (Garbini 1994; Lemche 2003), the view of Biran and Naveh that it is genuine and that this word should be translated “house of David” has been widely accepted. A second portion from the same inscription was published by Biran and Naveh in 1995, who also suggested how the two fragments might be attached to each other by means of a minimal join.

At the time of writing I am not aware that the fragments have been subjected to forensic examination, something that perhaps should now be routine in view of recent proven forgeries. While the problem arises chiefly with unprovenanced artefacts, the manner of the circumstances of the discovery of the Tel Dan fragments exhibit enough peculiarity to justify verification of their authenticity. I have no opinion or prejudice regarding their authenticity: but until reasonable doubts are removed, any conclusions are provisional, including those I shall offer in this essay.

The uncertainties facing the interpreter of these fragments are well known. In the first fragment of the inscription, the author states that an (unnamed) king of Israel had invaded the territory of his father, but that he had repulsed the invasion. The inscription is presumably some time after the mid-9th century, when (on the evidence of a major rebuilding phase) the city of Dan, perhaps previously independent, had come under Aramaean control. In the 8th century it was apparently to come under the control of Samaria (the kingdom of Israel).

There are numerous problems with the arrangement and interpretation of the two fragments (A and B). Despite their alignment by Biran and Naveh, it is not certain that the relationship between them has been correctly discovered: we cannot even be certain that these fragments are

contiguous, and therefore that they can be correctly positioned and thus that the contents of one can be directly related to the contents of the other (see Athas 2003, 191 for discussion). In the first fragment, the phrase “king of Israel” (*mlk yrs^{rl}*) is clearly legible, followed by a form of the verb “kill” (*qtl*); and in the next line *bytdwd*, with a *kaph* as the surviving last letter of the preceding word, which Biran and Naveh restore as [ml]*k*. Yet the phrase “king of the house of David” does not occur anywhere in the Bible, the only source in which “house of David” is attested. The second fragment contains the sequence *lyhw br[*, probably a personal name ending in a Yahwistic theophoric, followed by “son [of].” Biran and Naveh here supply [Amaz]iah son of Joash. The basis for this conjecture is biblical: Amaziah is mentioned in 2 Kgs 12–14 as a monarch of Judah at approximately the time of the events mentioned in the inscription; there is no epigraphical information beyond the suffix, and the person is not necessarily a king.

The use of biblical information here is understandable, but controversial, since historical correlations between the inscription and the biblical narrative are difficult. If *bytdwd* alludes to the ruling dynasty of Judah, then perhaps this battle is the one between Bar-Hadad II of Aram on the one hand and Ahab of Israel and Jehoshaphat of Judah on the other, described in 1 Kgs 22:29–38; or, as the editors prefer, a battle between Hazael of Aram on the one hand and Joram of Israel allied with Ahaziah of Judah on the other (2 Kgs 8:28–29; 9:14–16). Both battles took place, according to the biblical narrative, at Ramoth-Gilead in Transjordan, not in the vicinity of Dan. According to 1 Kgs 15:16–22 (= 2 Chr 16:1–6) a battle did take place between Bar-Hadad I of Aram and Baasha of Israel, in which Bar-Hadad captured Dan, along with other Israelite cities. However, Asa, the king of Judah, was, according to this report, on the side of Bar-Hadad I against Baasha—and his name as it occurs in 1 Kings does not fit the text of the inscription. Possibly, then, the reference is to a battle that is not mentioned in the Bible: Biran and Naveh do not express an opinion on this, but they do conclude that the author of the inscription was Hazael of Damascus. Athas, however, has argued for his successor Bar-Hadad (whom he calls “II”; Athas 2003, 263), who would have reigned at the beginning of the 8th century.

It is not my intention here to review any further the numerous proposals for the interpretation of these fragments (see the bibliography below). I remain in any case uncertain that “house of David” is the clear meaning of *bytdwd*, rather than one of the alternatives. Nevertheless, it is profitable to use that reading as a pretext to explore one implication that seems to have been taken for granted rather than examined—namely, whether *bytdwd* could have been a reference to a kingdom of Judah.

The argument in favour of this equation is generally based on Assyrian usage. It is well known that the common term for the kingdom of Israel in Assyrian records was *bit humri(ya)*, “house of Omri.” Shalmaneser III’s “Kurkh stele” mentions “Ahab the Israelite” is mentioned, while on the “Black Obelisk” Jehu “son of Omri” is mentioned (and depicted). The Nimrud slab inscription recording Adad-nirari III’s expedition to Palestine in 803 mentions Hatti-land, Amurru-land, Tyre, Sidon, *matHu-um-ri* (land of Omri), Edom, Philistia (Palestine?), and Aram (but not Judah). His “Rimah stele” mentions Joash of Samaria. Tiglath-pileser III (743–732) mentions “Menahem of Samaria” together with a list of other north Palestinian and Syrian kingdoms.¹ Finally, Sargon II describes his conquest of the city of Samaria, and also of “the whole house of Omri.” The regular Assyrian usage is thus “House of Omri” or “land of Omri.” The single case where “Israel” is mentioned is Shalmaneser’s account of his battle with the coalition at Qarqar, whose participants include *Ahabbu matSirila*. After the severe territorial reduction inflicted by Assyria, “Samaria” is used and the name is subsequently transferred to the Assyrian province.

But these references are not necessarily the most appropriate evidence. The Assyrian and Babylonian texts regularly refer to “Judah,” and never “house of David.” And, in any case, Akkadian usage is less relevant in this case than Syro-Palestinian. The Mesha stele and the biblical texts themselves presumably reflect the local usage rather more accurately. I begin with the Judean material.

There are 25 occurrences of the term “house of David” in the Bible. First Samuel 19:11 refers literally to David’s dwelling place and 1 Sam 20:16 to his family; 2 Sam 3:1, 6 refer to war between the “house of David and the “house of Saul”—again, two families are meant. Thereafter the name refers to the ruling dynasty. First Kings 12:19–20 (and 2 Chr 10:19) states that “Israel rebelled against the house of David” and distinguishes between the “house of David” and “Judah”; the former clearly means the dynasty or family, not the kingdom (the same is true of v. 26). First Kings 13:2 also refers to the royal house, as does 14:8. Second Kings 17:21 likewise. Isaiah 7:2 cannot denote the kingdom of Judah since “Judah” is named in the preceding verse; both this and v. 13 refer to the royal house, as in 22:22. Jeremiah 21:11–12 reads: “to the house of the king of Judah say: ‘Hear the word of Yhwh, O house of

1. The reference to *az-ri-a-u* (? ANET *ia-u-ha-zı*) *matia-u-da-a* is seen by a minority of scholars (see, e.g., ANET) as a reference to Azariah of Judah; the majority, however, identify the state in question as *Ydi*, mentioned in the Zinjirli inscription and located in northern Syria.

David...’” Here again, the “house of David” is the house of the king of Judah, namely the dynasty, but not the kingdom itself. In Zech 12:7–10 the “house of David” is again the ruling dynasty of Judah; in Ps 122:5 the meaning is probably the same (the verse speaks of the “tribes of Israel”); 1 Chr 17:24 the meaning is hardly “Judah,” nor in 2 Chr 8:11 (“house of king David of Israel”). Second Chronicles 21:7 has the dynasty in view also (“Judah” is used in vv. 8, 12, 17 of the kingdom). Nehemiah 12:37 refers to a building.

The term “house of David” is therefore never used in the Bible as a name for the kingdom of Judah. The most important texts are: 1 Kgs 12:20, “There was no one who followed the house of David except the tribe of Judah”; 12:26, “Now the kingdom may well revert to the house of David”; and 2 Kgs 17:21, “When he had torn Israel from the house of David, they made Jeroboam son of Nebat king.” *There remains a clear distinction between a royal dynasty, or even a royal family, and the kingdom over which it rules.* (Similarly, “house of X” is never used for the kingdom of Israel: instead “house of Ahab” [Mic 6:16] or “house of Jeroboam” [1 Kgs 13:34; 14:10–14, etc.; Amos 7:9–10]). The Assyrian inscriptions, on the other hand, either make no distinction or do not regard the distinction as relevant, since it is the royal house itself that they are confronted with. This is why the Akkadian usage is not relevant to the Tel Dan inscription.

Of course, the biblical usage may not reflect contemporary idiom, or may have been subjected to editorial modification. The Mesha inscription thus seems the most crucial in clarifying the issue. Mesha king of Moab (ca. 840) refers both to “Israel” and also “Omri” and his (unnamed) “son” (= a king of the same royal house). “House of Omri” does not appear as a designation. The personal name “Omri” appears in a slightly extended sense in lines 7–8: “Now Omri had taken possession of all the land of *mhdb* (= later Medeba). He lived in it during his days and half of the days of his sons—40 years.” But the use of the term “sons” (and “son” in line 6) argues against the equation of “Omri” with “house of Omri” here; it seems rather that we have an economical way of saying that Omri’s annexation was perpetuated through his sons. But even if we might (improbably) argue for “Omri” = “house of Omri” here, there is no question of equating “Omri” with the kingdom itself, as line 11 makes clear (“now the king of Israel had rebuilt Atarot for itself”: see also line 18). Note: not “king of the house of Omri!” See also line 14: “Go, capture Nebo from Israel!”

So much for the assumption that “house of Omri” is equivalent to “Israel,” or that “house of David” can be read as equivalent to “Judah.” If *bytdwd* at Tel Dan can be read as “house of David,” what might it

denote? Before answering that question, we should consider the suggestion that the term occurs also in Mesha's inscription. Lemaire has read one destroyed letter, the first *dalet* in "[D]avid" to restore a complete sentence in the latter part of line 31: "As for Horonen, there lived in it the house of [D]avid" (וּחוֹרֵן שָׁב בַּה בְּתִי דָּבֶד). This reading is not undisputed: Margalit (1994) has supplied *mem* instead of *dalet*, plus further conjectures and a translation: "Now Horoneyn was occupied at the en[d] of [my pre]decessor['s reign] by [Edom]ites," while Bordreuil (2001) has stated that he cannot confirm Lemaire's reading. Here again I am unable to reach an opinion, but if Lemaire were correct, could it be a reference to the kingdom of Judah, as he affirms? Given the distinct use of "Omri" and "Israel" *but not* "house of Omri," it must be doubted whether "house of David" should be equated with a kingdom of Judah: we would expect either "David" or "Judah." Again, as with the Tel Dan occurrence (and I perhaps need to restate here my uncertainty as to the validity of either reading) the advocates of the translation "house of David" need to explain what kind of entity they think it might refer to.

The phrase *bytdwd* is a valuable heuristic item, in fact, whether or not it is the correct reading in either of the inscriptions. The oddity of the usage (*pace* many commentators) draws our attention to the status of the territory designated as "Judah" at the time the inscription was created—the late 9th or early 8th century. As has been shown, there is no use of the term "house of X" in Syro-Palestinian inscriptions to denote states that are geographically defined, and the same is true of the biblical literature—without exception. The expression is rather used of families and dynasties—or, in cases where there is no state, a group exercising political and military control over certain people or territories—which corresponds more closely to what we might describe as a "chiefdom."²² (Such a configuration is reflected in the stories of Gideon, Abimelech, Jephthah, and the younger David.) The most likely interpretation of *bytdwd* is that it would refer to such a group: the restoration *mljk bytdwd* is out of the question on the linguistic evidence available. The use of the terms "Israel" and *bytdwd* in the same inscription (and, if we accept Lemaire's contested reconstruction, also in the Mesha stele) strongly suggests that

2. The definition of the term "chiefdom" is much contested and the literature, even in respect of ancient Israel and Judah, is extensive. Nor is the distinction between "chief" and "king"/"chiefdom" and "monarchy" always clear-cut. For the purposes of the present discussion, the essential difference is that the former entails those in allegiance to a chief, the latter a state that is territorially defined. I accept that this is neither an agreed nor a totally adequate definition: in effect it constitutes the working definition of "state" that is appropriate to Iron Age Palestine, including both city-states and so-called territorial states.

we are not *prima facie* dealing with equivalent or similar entities, that is, not two (neighbouring) *kingdoms* or *states*. In other words, to the writer of the Tel Dan stele, the *kingdom of Judah* is invisible. It does not, on the evidence, exist.

This interpretation can also be supported to some extent by the biblical narrative of Samuel–Kings, which presents David as an independent king of Judah already in the time of Saul, who then assumes the throne of Israel also: subsequently, the "united monarchy" of the two states falls apart when ten of the tribes "secede." The notion of a "united Israel" of twelve/thirteen tribes is assumed in the books of Genesis–Judges, but in Judges at least this notion has been isolated as an editorial layer, leaving a Hexateuch along with Chronicles as the sole biblical champions of such a "united" nation. The archaeological evidence, though not quite conclusive (as archaeological evidence rarely is), nevertheless tends very strongly in the direction of a separate origin of Judah and Israel from the time of their settlement in Palestine (see Finkelstein 1988; Davies 2007). While that conclusion would confirm the presentation in Samuel–Kings of two originally separate polities, the "house of Judah" and "house of Israel" (2 Sam 12:8 etc.), there remain further doubts about the probability that a Judean state can have come into existence at the same time as an Israelite one. Although the notion of a Davidic–Solomonic "empire" is agreed on all sides to be virtually impossible, a number of archaeologists believe that a powerful kingdom centred on Jerusalem in the 10th century may have existed.

In their recent book on the history of David and Solomon, however, Finkelstein and Silberman (2006) conclude that the stories of Saul and David are not contemporary and that the two characters, or their historical counterparts, were not, either. According to these authors, Saul and David were not rivals for the same "kingdom" (the word "state" is perhaps inappropriate at this stage of political evolution), though in the case of Saul the claim is more realistic, given the disparity between the population density, and economic and social development between the central and the southern highlands: "In the first half of the ninth century BCE Israel was one of the most powerful states in the region" at the time when the "first archaeological signs of state formation are evident in Judah" (Finkelstein and Silberman 2006, 98). They cite evidence of building projects at Lachish, Beth-Shemesh and in the Beer-sheba valley as evidence of the beginnings of a Judean state at this time. But whether these are signs of a centralized state (in Jerusalem) or of the kind of city-state revival that had apparently been taking place earlier in the north, before the campaign of Sheshonq, is not proven. Nor is it clear whether,

if there is a centralized state behind this building, that state is centred in Jerusalem or Samaria. The interpretation of the evidence is still strongly influenced by the biblical portrait.

And yet, according to the biblical records, Judah was in fact controlled by Israel during the 9th century, including a marriage of the daughter of the Samarian royal house to Jehoram. It is plausible—as Finkelstein and Silberman accept—that as soon as the population of Judah began to acquire political weight, the territory was subjugated. As Finkelstein and Silberman describes this era: “...this united monarchy—a real united monarchy—was ruled by the Omrides, not the Davidides, and its capital was Samaria, not Jerusalem” (2006, 103). The question raised by the word *bytdwd* at Tel Dan is whether Judah was in fact an independent kingdom at all at this point, or recognized as such. The fact that the Tel Dan and Mesha stelae make no reference to a kingdom of Judah justifies a doubt. It certainly does not follow that Judah was recognized by any of its neighbours as a kingdom until after the Assyrian depredations of the mid-8th century, which weakened Israel to the point where a Judean political dynasty could resist the call from its suzerain, Israel, in alliance with Aram, to join a coalition against Assyria, making instead a direct plea for protection to the Assyrian king. Whether there can have been a “king” of “Judah” before Ahaz is questionable; it is more probable that its “king” was in fact the monarch of Israel.

The stories of David—on this interpretation of both biblical and archaeological evidence—remain largely fictional, but they do not rule out the memory of the name of the chiefdom that later assumed the monarchy of Judah, nor of the eponymous founder (and his successors) as chieftains who, as vassals of the king of Israel, had local control of the southern central highlands. On the basis of the *external* evidence, it may be possible to pinpoint more accurately the moment at which a kingdom of Judah assumed an independent status. But at the time of writing of the Tel Dan stele, while *bydwd* might yet be read as a reference to a vassal chiefdom of the kingdom of Israel, it remains to be argued that this chiefdom can be identified as a kingdom of Judah, or its leader as a *melek*. On the contrary, the evidence may well support the view that the biblical profile of “King David” is indeed fictional.

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FROM MERNEPTAH TO SHOSHENQ: IF WE HAD ONLY THE BIBLE...*

Lester L. Grabbe

In several previous studies I have attempted to address the question, "What might be known about the history of Israel if we had only the Bible?" That is, how much of the biblical text is reliable history and how much is material with another purpose but cannot be taken as conveying reliable historical data? We now come to one of the more problematic periods of Israelite history as far as trying to determine what happened. It is also widely accepted that many of the biblical accounts of this period are not good historiographical sources, but the question of how helpful they are needs discussion.

The present study will survey the sources for the period about 1250–850 B.C.E., discuss this history for this period in the light of past scholarship, and finally draw some conclusions about the value of the biblical material. The present study is arranged as follows:

I. Sources

Archaeology

Terminology of Archaeological Periods

The Low Chronology

Archaeological Survey (LB to Iron IIA)

Analysis

Medinet Habu and Related Inscriptions

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Joshua

Judges

Saul, David, and Solomon Traditions (1 Sam 13 to 2 Kgs 11)

* An earlier version of this study was published as Grabbe 2007b. The present version has now been thoroughly revised and updated.

1. See Grabbe 2005a, 2007b; cf. 1997, 1998.

II. Analysis

The Question of the Exodus

The Sea Peoples and the Philistines

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I. Sources

Archaeology

Terminology of Archaeological Periods. A number of slightly different archaeological schemes, divisions, and datings are used. One scheme of the EB and MB (Albright 1932, 8–18; *NEAEHL* 4:1529) divides EB I (ca. 3300–3000), EB II (ca. 3000–2700 B.C.E.), EB III (2700–2200 B.C.E.), MB I (2200–2000 B.C.E.), MB IIA (2000–1750 B.C.E.), MB IIB (1750–1550 B.C.E.), or possibly three MB divisions: MB IIA–C. However, MB I is often labeled EB IV or Intermediate Bronze (Dever 1977, 82–89; Finkelstein 1995a, 87–88), in which case the MB (Dever 1987, 149–50; Ilan 1998, 297) is divided into MB I (ca. 2000–1800 B.C.E.), MB II (ca. 1800–1650 B.C.E.), and MB III (ca. 1650–1500 B.C.E.), though some want to divide MB only into MB I and MB II. In spite of these differences, this concerns only the background history of the second millennium B.C.E.

For the history of ancient Israel, a major issue arises over the end of the LB, beginning of the Iron I, and end of the Iron I (also, some divide Iron I into IA and IB). This, though, is not the main difficulty. The real problem is that at the moment there is no agreed terminology among archaeologists for the period from about 1000 B.C.E. to the fall of Jerusalem in 587/586 B.C.E. Some schemes are based on supposed "historical" periods, which can be question begging since most schemes tend to associate certain artifacts with particular historical periods. After the Hazor excavations of the 1950s (so Barkay 1992, 305), Aharoni and Amiran proposed Iron II (1000–840 B.C.E.) and Iron III (840–586 B.C.E.), but the *Encyclopedia of Archaeological Excavations in the Holy Land* (when first published in Hebrew in 1970 but also maintained in the English translation [Avi-Yonah and Stern, eds., 1975–78, 4:1226]), had the following scheme: Iron IIA (1000–900), IIB (900–800), IIC (800–586). W. G. Dever notes that American archaeologists tend to begin Iron II about 920 B.C.E., whereas Israeli archaeologists usually begin it about

1000 B.C.E. (Dever 1998b, 416). His own treatment ends Iron IIA at 722 and Iron IIB with 586 B.C.E. However, the editor of the volume to which Dever contributes (Levy 1998, x–xvi, especially xvi) divides as follows: IIA (1000–900); IIB (900–700); IIC (700–586). But even among Israeli writers the Iron II is differently divided. Barkay proposed Iron IIa (10th–9th centuries B.C.E.), Iron IIb (8th), IIIa (7th and early 6th), IIIb 6th (586 to the late 6th B.C.E.). The *NEAEHL* used the terminology Iron IIA (1000–900 B.C.E.), IIB (900–700), IIC (700–586), Babylonian and Persian periods (586–332). However, A. Mazar (1993) divided slightly differently, with Iron IIA (1000–925 B.C.E.), IIB (925–720), IIC (720–586). H. Weippert (1988) differed from them all, with Iron IIA (1000–900 B.C.E.), IIB (925/900–850), IIC (850–587).

This only partially controlled chaos means that readers must always pay attention to the scheme used by the individual author, and the individual author ought (though not necessarily does so) to indicate to the reader the particular scheme being used. The present study is divided into chapters that cover particular periods of time, without being watertight compartments. But because it draws on a great variety of archaeological writings, it will quote the data according to the scheme used by the particular writer. I have tried to give preference to the archaeological period (LB, Iron I, Iron IIA, etc.) rather than a date; however, if a specific date or even century is given, an indication of who is doing the dating is normally provided. This has been complicated by the Low Chronology, since material culture conventionally dated to the 10th century B.C.E. would usually be dated to the 9th according to the Low Chronology.

The Low Chronology. One of the most controversial but also potentially important developments is Israel Finkelstein's "Low Chronology" (LC). His original study (Finkelstein 1996a) argued that conventional chronology is based on the twin pillars of the stratigraphy of Megiddo and the Philistine Bichrome Ware. Since then the debate has widened considerably to take in radiocarbon dating, correlation with the Aegean, the context of the Assyrian expansion in the 9th century, and other factors. A major opponent of the LC has been Amihai Mazar (2005) who has developed what he calls the "Modified Conventional Chronology" (MCC). This is similar to the traditional chronology but extends the Iron IIA from 980 to 830, that is, covering most of both the 10th and the 9th centuries. This allows three major pottery periods (Iron IB, Iron IIA, Iron IIB), each lasting about 150 years, in the 450 years between approximately 1150 and 700 B.C.E.

The following are some of the main issues around which the arguments—pro and con—have revolved:

1. The biblical data. Finkelstein points out that conventional dating is strongly—if not always explicitly—influenced by the biblical text (e.g. 2005a, 34). This sometimes leads to circular arguments in which the argument depends on the biblical text and moves around to arguing that the data support the biblical text. A good example is Shoshenq's invasion (see next point). Yet all parties have appealed to the biblical text in the discussion of the two key sites of Samaria and Jezreel (Ussishkin 2007b; Finkelstein 2005a, 36–38).

2. The invasion of Shoshenq. This event has been a central benchmark for dating historical and archaeological data, yet the dating of when it happened has depended on the biblical text, since 1 Kgs 14:25–28 puts it in the 5th year of Rehoboam (Finkelstein 1996a, 180). A recent Egyptological evaluation has dated the event to 917 B.C.E. (Shortland 2005). Yet this is not the end of the matter, because it is not clear exactly what sort of measures were taken by Shoshenq. It had been assumed that his invasion resulted in widespread destruction of sites, but it has been argued that if Shoshenq planned to use Megiddo as the place to plant his royal stele, he would hardly have destroyed the site (Ussishkin 2007a). In fact, it "has not been proven that any sites were destroyed by Shishak in 925 B.C.E., and the attribution of destruction layers to the end of the 10th century at many sites is mere conjecture" (Barkay 1992, 306–7).

3. Implications for the Aegean and other Mediterranean areas. N. Coldstream (2003, 256) argues that the LC best fits the situation in the Aegean, but A. Mazar (2004) stands against this. Yet Killebrew (2008) also relates the situation in Philistia to the broader Mediterranean pottery context and agrees that the LC "with some minor revisions" would best fit the situation elsewhere in the Mediterranean (see next point).

4. The development and dating of Philistine Monochrome and Bichrome pottery. It seems to be generally agreed that Philistine Monochrome (or Mycenaean IIIC:1b or Mycenaean IIIC Early to Middle) appears in the late 12th century, developing from imported Mycenaean IIIC:1a (Mycenaean IIIC Early or Late Helladic IIIC Early). The 20th Egyptian Dynasty and the Egyptian presence in Palestine came to an end about 1140/1130 (Mazar 2007). Yet no Philistine Monochrome Ware appears together with Egyptian pottery of the 20th Dynasty, not even in nearby sites such as Tel Batash and Tel Mor, which suggests that Philistine Monochrome is post-1135 B.C.E., and the Philistine Bichrome is even later—the 11th and perhaps much of the 10th century but not before 1100 B.C.E. (Finkelstein 1995b, 218–20, 224; 2005a, 33). A. Mazar (2008) responds: (a) Canaanite pottery assembly in Ashdod XIII and Tel Miqne is typical of the 13th and beginning of the 12th centuries B.C.E., and Lachish VI must have been contemporary; (b) local Mycenaean IIIC

is inspired by the Mycenaean IIIC pottery in Cyprus but this disappears after the mid-12th century; (c) although D. Ussishkin and Finkelstein claim it is inconceivable that locally made Mycenaean IIIC did not reach contemporary sites in Philistia and the Shephelah, this ignores cultural factors that could limit it to a few urban centres; the early stage of Philistine settlement lasted perhaps only a generation, and Lachish is at least 25 km from the major Philistine cities which is sufficient to create a cultural border; (d) the Philistine settlement was possible perhaps because of the state of the Egyptian domination of Canaan at this time, and Philistine Bichrome pottery slowly emerged as a hybrid style later, probably during the last quarter of the 12th century.

Yet in a thorough study of the Aegean-style pottery A. E. Killebrew (2008) argues that the high chronology (two-wave theory) would date the Philistine Monochrome (Mycenaean IIIC:1b) to about 1200 B.C.E. as the result of an early proto-Philistine wave of Sea Peoples, while the middle chronology would date this pottery to about 1175 B.C.E., with the Bichrome developing from it in the mid-12th. Such datings are becoming more problematic in light of the increasing consensus that Mycenaean IIIC:1b should be equated to Mycenaean IIIC Early (into Middle) which is dated to the mid-12th. With some revisions (such as dating the initial appearance of Mycenaean IIIC Early phase 2 to about 1160 B.C.E.), the low chronology would best fit the dating of Mycenaean IIIC Early to Middle at other sites in the eastern Mediterranean and would also provide a more reasonable dating of Bichrome to the 11th continuing into the 10th, based on the LB II–Iron I stratigraphic sequences at both Tel Miqne and Ashdod.

5. Pottery assemblages and the dating of various strata. Here there is a surprising difference of interpretation between professional archaeologists whom one would expect to agree about the facts in the ground: the relationship between the strata in the sites of Hazor, Samaria, Jezreel, and Megiddo; the interpretation of Jerusalem; the dating of Lachish; the dating of the Negev destruction. See below for some further discussion of these.

6. The gap in the 9th century. Current dating leaves a strange gap in the 9th century in the dating of archaeological remains (Finkelstein 1996a). We have much in the 10th and the 8th centuries but not the 9th, leaving a very thin stratigraphy over 350 years. For example, Tel Miqne and Tel Batash have thick accumulations related to Philistine Bichrome Ware, a 9th-century gap, then limited Iron II remains. Other sites show a similar gap: Tell Halif, Tel Mor, Tell Beit Mirsim, Ashdod, Tel Haror, Gezer, Jerusalem. The LC closes the unexplained gap between monumental

architectures of the 10th century and evidence of public administration for the late 9th to the 8th centuries B.C.E.

7. Downgrading or elimination of the United Monarchy. The monuments previously associated with the United Monarchy are redated from the second half of the 10th century to the early 9th. It strips the United Monarchy of its monumental buildings, including ashlar masonry and proto-Ionic columns. We have evidence of fortifications in the 10th century, but the main mounds in the north (Megiddo and Gezer) and the south (Beersheba and Lachish) only date to the 9th century or later. This means that the strong and historically attested Omri kingdom is the first state in Palestine and preceded the geographically weaker Judah. Taking a global perspective, this is what one would expect—rather than the anomalous Jerusalem-centred and Judah-dominated kingdom of David and Solomon.

8. Radiocarbon dates. This is the most recent attempt to find a way to pin down the matter of dating and has great potential. Yet a database of radiometric dates from a wide variety of sites is needed; such is now being developed and may resolve the issue (Sharon et al. 2005, 2008). In the meantime, there are significant differences in interpretation. The reasons for this have been well laid out by A. Mazar (2005, 22):

The many stages of selecting the samples, the pre-treatment, the method and process of dating, and the wide standard deviation of Accelerator Mass Spectrometry dates may create a consistent bias, outliers, or an incoherent series of dates. The calibration process adds further problems, related to the nature of the calibration curve in each period. In our case, there are two difficulties: one is the many wiggles and the shape of the curve for the 12th–11th centuries B.C.E. This leads to a wide variety of possible calibrated dates within the 12–11th centuries B.C.E. The other problem is the plateau between 880 and 830 B.C.E., and the curve relating to the last third of the 10th century B.C.E., which in certain parts is at the same height as the 9th century B.C.E. plateau. In many cases the calibrated dates of a single radiocarbon date is in both the 10th and the 9th centuries B.C.E., and in the 9th century B.C.E. more precise dates between 880 and 830 B.C.E. are impossible. These limitations are frustrating, and make close dating during this time frame a difficult task. It seems that in a debate like ours, over a time-span of about 80 years, we push the radiometric method to the edges of its capability, and perhaps even beyond that limit.

The result is that a variety of radiocarbon dates have been made at key sites, with some arguing that they support the MCC (Mazar 2005, 22–23; Mazar et al. 2005); and others, that they support the LC (Finkelstein and Piasezky 2003; Piasezky and Finkelstein 2005; Finkelstein 2005b; Sharon et al. 2005, 2008).

Archaeological Survey (LB to Iron IIA). Because of the debate over chronology, the following archaeological survey will attempt as far as possible to refer to archaeological periods (Iron I, Iron IIA, Iron IIB) rather than specific dates.² The stratigraphic and chronological framework for the Iron Age in Palestine is based on several key sites: Megiddo, Lachish, Jerusalem, and Samaria (Ussishkin 2008). Jerusalem is probably the most contested site of all those in the whole of Palestine. It is vital for the period covering the so-called “United Monarchy.” Finally, as noted above, the invasion of Shoshenq has been used extensively to date specific sites with a destruction layer. Yet as Ussishkin suggests, Shoshenq’s treatment of various sites may have differed; for example, the stele erected in Megiddo suggests a site that he occupied rather than destroyed.

The LB period was one which S. Sherratt unapologetically refers to as a period of “globalization” (Sherratt 2003). In Palestine this miniature golden age came to an abrupt end sometime about 1200 B.C.E. with the destruction of the main centres: Megiddo, Beth-shean, Lachish, Ashdod, and the like. Yet the “conventional wisdom” that the Palestinian city-state system came to an end oversimplifies what happened (Finkelstein 2003, 75–79). For the rural sector experienced continuity, both demographically and culturally, as is indicated by such sites as Tel Menorah, Tell el-Wawayat, and ‘Ein Zippori. The peasants in the vicinity of the ruined LB cities lived as they already had, and the northern valleys remained densely settled. In the Iron I (11th century, according to Finkelstein) the main centres began to recover, the main exceptions being Lachish and Hazor. This “new Canaan” was prosperous because of stability in the rural sector and trade with Phoenicia, Cyprus, and even further afield.

If we begin with the site farthest north, the current excavators of Hazor have retained Y. Yadin’s assignment of stratum X to the 10th century and stratum VIII to the Omride dynasty (Ben-Tor 2000; Mazar 2008). Although some ¹⁴C dates contradict this, Mazar notes that it seems difficult to compress strata XB to VIII into 70 years in the 9th century, as required by the LC (see Finkelstein’s response in 2005a, 38). The conventional view equates Megiddo VA–IVB, Hazor X, Gezer VIII, and Beersheba V—all seen as evidence for the United Monarchy

2. Note that the LC and MCC coincide from the 8th and 7th centuries onward (Finkelstein 1999a, 39). It is generally agreed that Iron IIB ends, and Iron IIC (or, as some prefer, Iron III) begins, with the fall of Samaria and/or invasion of Sennacherib (ca. 720/701 B.C.E.). See the charts in Mazar 2005; cf. Ofer 2001, 30–31.

(Finkelstein 1996a, 177). Thus, according to Mazar (2008), Yadin’s thesis of Solomonic architecture at the three sites (Megiddo, Hazor, and Gezer) might still be correct. Finkelstein (1999b, 59) argues, however, that Yadin’s equation of Hazor X with the time of Solomon creates chaos with strata IX, VIII, and VII, leaving no place for the important activities of Hazael in northern Israel. A. Zarzeki-Peleg’s study of the pottery assemblages connects Megiddo VIA with Jokneam XVII, while Hazor XB is later (1997, 284). Thus, Finkelstein (1999b, 60) argues, Hazor X was built by the Omrides.

If we move back to the LB, A. Ben-Tor (1998) has been interpreted as still being of the opinion that the Israelites destroyed Hazor XIII and then settled it in stratum XII (thus continuing to agree—more or less—with Yadin’s interpretation); however, he has recently denied this (Ben-Tor and Zuckerman 2008, 2). In any case, D. Ben-Ami (2001) gives a somewhat different interpretation. He makes several corrections to previous interpretations (2001, 165–70, my formulation). First, the destroyers of LB Hazor (stratum XIII) left the site desolate and uninhabited. However, the “impressive Late Bronze city of Hazor with its magnificence, monumentality, high status and influence...underwent a violent destruction that apparently cannot be related to the process of early Israelite settlement in Canaan” (Ben-Ami 2001, 168–69). Secondly, there was a gap in settlement for a period of time. Thirdly, the settlement of Iron I was a temporary encampment whose presence was indicated by a large number of refuse pits. Fourthly, although the identity of those in this encampment is unknown, the material culture is new, with substantial differences that suggest a different population. Fifthly, Yadin’s identification of two separate settlement strata (XII and XI) is unsupported and only one occupational phase is indicated by the remains.

In the upper Galilee the ceramic continuity from the LB II to Iron I indicates that the population was indigenous rather than immigrants, as pictured by the biblical text (Josh 19:24–48; Bloch-Smith and Nakhai 1999, 81). Settlement of the Jezreel and Beth-shean valleys flourished in the LB, with a significant Egyptian presence at the administrative centres of Megiddo and Beth-shean. The collapse of Egyptian dominance resulted in a general decline, with an impoverished culture (Bloch-Smith and Nakhai 1999, 81–88). A number of small settlements existed in this area, the majority established in the 12th and 11th centuries, with a material culture suggesting continuity with the LB (Gal 1992, 84, 92). The suggestion that a new population took control (Bloch-Smith and Nakhai 1999, 83) appears to have little or no archaeological support.

LB II Megiddo (strata VIII/VIIB–VIIA), with an estimated area of 11 ha, was one of the most prominent cities of Palestine (Finkelstein, Ussishkin and Halpern 2000, 593). With its monumental buildings it appears to have been the city-state centre for the king and his elite supporters. Megiddo thus seems to provide a good example of the nature of the major city-states in Canaan during the later part of LB (over 30 sites were apparently subordinate to Megiddo), though the LB city was apparently unfortified. The absolute dating of the LB II strata is not clear: according to Finkelstein, stratum VIII can hardly post-date the mid-14th century; it is unlikely that there was no palace during the Amarna period. Stratum VIIB alone would represent the city of the 13th century. Stratum VIIA was then built in the late 13th century and destroyed in the second half of the 12th. Strata VIII–VIIA therefore all represent one phase of urban continuity. Ussishkin agrees that Megiddo was continuously settled throughout the LB, including the Amarna period, but the character of settlement was not uniform throughout the time. Significantly, most or all the monumental buildings were constructed after the Amarna period, while the Amarna settlement was relatively modest. The conclusion seems to be that the stratum VIII royal palace and gatehouse and some other public buildings were constructed in the 13th century.

Megiddo VIIA was destroyed in the second half of the 12th century; rebuilt as VIB in Iron I it began to prosper again in VIA (2000, 594–95). This city was destroyed in a major conflagration. Megiddo VB was very different in character, with Iron Age traits in its material culture and layout (2000, 595–96). In the transition from VIA to VB there is a change both in the pottery tradition and the layout of the city; nevertheless, it remained an imposing administrative centre (2000, 597). Ussishkin (2007b) argues that Megiddo stratum VA–IVB or parts of it preceded the Omri dynasty and that this stratum (or the preceding stratum VB) was probably the city captured by Shoshenq in the late 10th century; however, Shoshenq did not destroy the city but intended to make it his administrative centre. (For a discussion of Megiddo in relation to Samaria and Jezreel, see Grabbe 2007a, 123–28.)

Thus, it seems that at least in the northern part of the country, LB Canaan rose again in the late 11th and early 10th centuries from the blow of the mid-12th century (Finkelstein 2003, 77). Other city-states seem to have emerged at that time, at Tel Kinneret, Tel Rehov, Tel Dor, and possibly Tel Keisan. Iron I Kinneret replaced LB Hazor as the centre of the upper Jordan Valley, while Iron I Tel Rehov was probably the focus of a territorial entity that covered the Beth-shean and eastern Jezreel Valley. LB Tel Keisan continued to prosper and seems to have served as

the central site of the northern coastal plain (including Akko, and Tell Abu Hawam IV as its port). Iron I Dor dominated the coastal plain of the Carmel Ridge and possibly replaced LB Gath (Jatt) as the main centre of the region.

It has long been suggested that a number of innovations in technology took place in the central highlands: terracing, plastered cisterns, the “Israelite house,” collared-rim jars, and iron (Dever 2003, 113–25). Unfortunately, it seems there is no list of technologies that is exclusive to this region and time or that can serve as “Israelite ethnic markers” (Killebrew 2005, 171–81; Bloch-Smith 2003, 406–11; Kletter 2006, 579; Hesse and Wapnish 1997). Terracing was used as early as the EB; the early Iron Age settlers in the highlands made use of an existing technology (Gibson 2001). It is now argued that the collared-rim jar was already in use in the 13th century along the Levantine coast and is the product of a particular lifestyle or economy (such as the distribution of rations to employees) and is not exclusive to the Cisjordan highlands or Israel (Killebrew 2001; Raban 2001; Herr 2001). A. Faust and S. Bunimovitz (2003) recently argued that the four-room house was particularly Israelite, though they admit that a few such houses occur elsewhere, including in Transjordan. Iron seems to have been little used, despite the name “Iron I”: weapons and tools were mainly bronze (Bloch-Smith 2003, 417–20).

In this context of unique technologies, the question of pork consumption has also often been discussed, as an ethnic indicator. The subject is a complicated one, though there is a considerable drop in evidence for swine in the highlands during Iron I and Iron II, in contrast to the LB and also the coastal plain where pigs remained a part of the diet. But Hesse and Wapnish conclude that “no human behavioral evidence exists to indicate that pig avoidance was unique to any particular group in the ancient Near East... Lots of people, for lots of reasons, were not eating pork” (1997, 261). S. Bunimovitz and Z. Lederman (2008) found that the total absence of pig bones in Beth-shemesh in Iron I was consistent with the minimal percentage of pig bones in the hill country, in contrast to sites identified as Philistine. Beth-shemesh became a sensitive seismograph for social changes between the regions, one such conspicuous boundary being pork avoidance. This allows that the pig taboo as an ethnic marker might have begun in the Shephelah and extended into the highlands, rather than the other way round.

For the area of Ephraim we have the preliminary survey results (Finkelstein 1988–89, especially 144–54; Finkelstein 1988, 121–204). The LB had seen a demographic decline, but the Iron I produced a



"settlement wave of unprecedented intensity," especially in the desert fringe and the northern central range (Finkelstein 1988–89, 146). This produced a pattern of major centres accompanied by a peripheral population. Finkelstein (1988–89, 148) argues that the population was densest in the east at the beginning of Iron I but then moved toward the west, with the cereal/animal husbandry economy also shifting toward a horticultural one; however, Bloch-Smith and Nakhai (1999, 71) assert that the archaeological data do not support either of these conclusions. Yet a comparison of Iron I and Iron II does indicate just such a population shift (Finkelstein 1998a, 357). Regardless, it does appear that the Ephraimite and Manassite regions had the densest population of any region west of the Jordan. Settlement of the less favourable slopes and foothills, with trees and brush to be cleared, came later in the Iron I and continued into Iron II. In Iron II almost all the regions of Ephraim were intensively settled (Finkelstein 1988–89, 151–54). Compared with Iron I, the population had shifted west, with some of the large sites in the east abandoned: Shiloh, 'Ai, and Khirbit Raddana. Sufficient grain was grown in some regions, apparently, to allow the western regions to concentrate on the important wine and oil production. In the southern central range of the Ephraimite hills some sites were abandoned and a fall in the population generally occurred in this region.

In the LB and early Iron I, a clear difference separated the north and south in the central hill country between the Jezreel and Beersheba valleys (Finkelstein 1998a, 361; 1999a, 43–44). The north experienced significant continuity, with most of the main sites continuing from the LB into Iron I. The Negev and Judaean Hills had hardly any sedentary sites in the LB; this long settlement gap came to an end in the early Iron I. There was a large increase in small sites in the early Iron I, especially in the central hill country (Stager 1998, 134–37; Dever 2003, 97–100). This increase was largely in the north, especially in the areas of Ephraim and Manasseh, and almost entirely north of Jerusalem. The Judaean Hills had hardly any new sites in the early Iron I, and the region still remained highly pastoral (Finkelstein 1988, 52–53).

With regard to the Judaean Hills, however, we find a significant difference of opinion. Most of the early Iron I sites were between Jerusalem and Hebron, with hardly any south of Hebron. According to A. Ofer, the mid-11th to the 10th centuries saw a rapid growth in the Judaean Hills, including the more peripheral areas (Ofer 1994, 102–4). Based on his survey results, Ofer (1994, 2001) argued that from the mid-11th to the 8th century the population nearly doubled in each century. To Iron IIA (a single identifiable ceramic stage) are to be assigned Tel Qasileh XI–IX, Izbet-Sartah II–I, Beersheba VIII–VI, Tel Edar III–II, Arad XII–XI (Ofer

2001, 30–31). We see the beginning of settlement in the pasture areas, and the first inhabitants of the desert fringe sites (e.g. Tekoa, ha-Qain, Ma'on, Carmel). The Shephelah only began to be populated at this date.

Some of Ofer's interpretation has been challenged. G. Lehmann (2003) criticized Ofer's survey from a methodological point of view (citing a recent publication of A. Faust). He noted that deciding between the conclusions of Ofer and Faust was currently impossible because the political situation prevented the necessary testing. Nevertheless, Lehmann (2003, 133, 141, 157) went on to argue, in contrast to Ofer, for a much smaller settled area and a population more like 10,000 in Iron I and 16,000 in Iron IIA Judah (including Jerusalem), but these are maxima: he reckoned Iron IIA at 5000 to 10,000, with Iron I correspondingly less. The built up area of the Shephelah was twice as large as Judah in Iron I and perhaps even three times as large in Iron IIA (2003, 134). Yet Z. Herzog and L. Singer-Avitz (2004, 220) critiqued Lehmann for nevertheless accepting too many of Ofer's data without question, though their study confirmed the view that the highlands of Judah (including Jerusalem—see below) were relatively sparsely settled, in contrast to the lowlands.

From a broader perspective the population density of the Judaean Hills was significantly less than the areas to the north. Ofer (1994, 107) notes the difference between the results of the Judaean Hills survey and those of the Manasseh hill country and the Land of Ephraim surveys. In Iron I Ephraim and Manasseh were four times that of the Judaean Hills, and twice that in Iron IIA. In the northern highlands most of the LB sites continued to be inhabited in Iron I, in contrast to the Judaean highlands which had almost no LB settlements (Finkelstein 1999a, 43–44). Many highland settlements disappeared in the later Iron I (Bloch-Smith and Nakhai 1999, 78). Growth in the Judaean Hills was slow from Iron I to Iron IIA, in contrast, for example, with Benjamin where the growth was 243 percent (Lehmann 2003, 134). Penetration into the desert fringe and an increase in the population of the southern regions of the Judaean Hills in general characterized the Iron IIA (Ofer 1994, 104). The continuity of settlement from Iron I to Iron II was over 70 percent, but the villages increased in Iron II, with 60 percent new (Lehmann 2003, 147–50). Unlike Iron I, the sites were not at the highest elevation but tended to be in the middle slopes, suggesting that the settlers were feeling more secure. The more important settlements lined up with the north–south watershed in both the Iron I and IIA. The settlement peak came about in the 8th century (Ofer 1994, 105). This all suggests that developments in the central territory of Judah lagged significantly behind those of the north, which has implications for evaluating state development in general, and the United Monarchy in particular.

As noted above, Jerusalem is probably the most contested site in Palestine. The main question is what kind of a settlement Jerusalem was in Iron IIA: Was it a minor settlement, perhaps a large village or possibly a citadel but not a city, or was it the capital of a flourishing—or at least an emerging—state? Assessments differ considerably, with Ussishkin, Finkelstein, Steiner, Herzog, Lehmann, and Singer-Avitz supporting a minimal settlement; but many (including A. Mazar and, especially, Cahill) arguing for the latter. Using Rank Size Theory Ofer had argued that, for the expected pattern to be followed, the graph of settled sites required Jerusalem to be included as the capital for the Judaean hill country (1994, 97–98, 102–4); however, Lehmann's Rank Size analysis shows just the opposite (2003, 149–51). D. W. Jamieson-Drake (1991) was one of the first who queried the status of Jerusalem, concluding that it did not have the characteristics of a capital city, including monumental architecture, until the 8th century B.C.E. Jamieson-Drake has been much criticized for lacks in his data, but it seems that he was right as far as he went, though he gave an incomplete description (Steiner 2001, 284).

J. M. Cahill (2003, 2004) has been one of the most vociferous voices arguing for a substantial city as early as the 10th century. She dates the “stepped stone structure” to the Late Bronze/early Iron transition, arguing that both the stepped mantle and the terraces below it were built together as a single architectural unit (Cahill 2003, 42). The fortification wall built during the Middle Bronze remained standing and was repaired and used until the Iron IIB (2003, 71). Cahill summarizes her conclusions for the 10th century (2004, 20):

My own view is that the archaeological evidence demonstrates that during the time of Israel's United Monarchy, Jerusalem was fortified, was served by two complex water-supply systems and was populated by a socially stratified society that constructed at least two new residential quarters—one located inside and the other located outside the city's fortification wall.

Reinforcing Cahill's interpretation, it has recently been argued that a building at the top of the “stepped stone structure” dates to David's time and could be his palace (E. Mazar 2006, supported by A. Mazar). The problem has been to obtain the proper archaeological details. Now, the relevance of E. Mazar's “palace of David” has been challenged (Finkelstein et al. 2007). Finkelstein, Herzog, Singer-Avitz, and Ussishkin argue that the building is most likely late Hellenistic and that Mazar's interpretation was primarily based on the Bible and not the archaeology:

First, it seems to consist of several elements, mainly a rectangular building in the west and the citywall in the east. Second, all one can safely say is that its various elements post-date the late Iron I/early Iron IIA and predate the Roman period. Circumstantial evidence seems to suggest the dating of most elements to the late Hellenistic period. Beyond archaeology, one wonders about the interpretation of the finds. The biblical text dominates this field operation, not archaeology. Had it not been for Mazar's literal reading of the biblical text, she never would have dated the remains to the 10th century B.C.E. with such confidence. This is an excellent example of the weakness of the traditional, highly literal, biblical archaeology—a discipline that dominated research until the 1960s, that was weakened and almost disappeared from the scene in the later years of the 20th century, and that reemerged with all its attributes in the City of David in 2005. (Finkelstein et al. 2007, 161–62)

The “stepped stone structure” is itself the subject of considerable dispute, with several positions being taken (Lehmann 2003, 134–36). In contrast to Cahill, Steiner (1994; 2003b, 351–61; 1998) argues from Kenyon's excavations that the stepped stone structure was quite different in extension and construction method from the terrace system. Where the terraces existed, only a mantle of stones was added, but where there were no terraces the structure was built up from bedrock. The dating and construction is confirmed by the pottery in the fill of the terraces which was Iron I, with no mix of later material. Steiner would date its construction to the 10th or early 9th century. In sum, the terrace and stepped structure systems do not have similar boundaries, identical pottery, or the same construction techniques. From Kenyon's excavations she feels there is enough evidence to show that the structure went out of use in late 8th or early 7th century B.C.E., when a new city wall was built further down the slope.

As described by Steiner (2001, 2003a, 2003b), Jerusalem of the 10th and 9th centuries was a small town occupied mainly by public buildings, not exceeding 12 ha and approximately 2000 inhabitants (compare this with Lehmann's 2 ha and 300–600 inhabitants in Iron I, and 4 ha and 600–1200 in Iron IIA [Lehmann 2003, 135–36]). It exhibits the characteristics of a regional administrative centre or the capital of a small, newly established state, the towns of Megiddo, Hazor, Gezer, and Lachish showing similar characteristics at the same time. Excavations on the Ophel show the earliest buildings there date only from the 9th century. E. Mazar had argued that the fortified complex of this area south of the Temple Mount had been constructed as early as the 9th century, but they more likely date between the 8th and the early 6th centuries B.C.E. (Killebrew 2003, 336). E. A. Knauf (2000) argues that the centre of the Davidic city has not been found because it would have been the

area north of the Ophel, the area of the Temple Mount. Although it is not possible to test this hypothesis now, that section of the hill was a militarily strategic area and would have had to be incorporated into any settlement on the south-eastern hill.

The argument that the Middle Bronze wall was used as a city wall in the LB, Iron I, and Iron IIA and IIB has no archaeological support: Jerusalem lacked a fortification wall until the mid-8th century, when the MB IIB wall was partially built over and partially reused for a new fortification wall (Killebrew 2003, 334; Ussishkin 2003, 110–11). The lack of other finds relating to fortification suggests that Jerusalem was unwalled and unfortified between the LB and Iron IIB (16th to mid-8th), and thus Jerusalem was “at best modest” (Killebrew 2003, 334). Also, the elaborate water system of MB IIB went out of use until the 8th /7th centuries (Warren’s shaft never served as a water system), as shown by the excavations of Reich and Shukron (Killebrew 2003, 334–35).

In the transition from LB to Iron I the Shephelah suffered a massive demographic decline, with 67 sites in the LB but 25 in Iron I, and a reduction of settled area from 105 ha to 60 ha (Finkelstein 1999a, 44). The Shephelah sites seem to have prospered in the later Iron I (Bloch-Smith and Nakhai 1999, 102–3). A number of lowland features of Iron I later became characteristic of highland Iron II, primarily the collared-rim storage jars, the four-room house, and the bench tombs. This indicates a movement of some lowland peoples to the east and north (Bloch-Smith and Nakhai 1999, 103). Y. Dagan (2004, 2680–81) concluded that in Iron IIA the sites developed slowly as the process of Judaean settlement began, with many “dispersed” and “isolated” structures, a situation unknown in earlier periods, indicating a period of stability. With regard to the central site of Lachish, it was rebuilt after a long habitation gap in Iron I (Ussishkin, ed., 2004, 1, 76–87). Although little remains of this city (level V), the inhabitants seem to be a new people with a new material culture. Some have wanted to date level V to the 10th century and connect it with the invasion of Shoshenq, but the latter’s inscription does not mention Lachish (nor any place in Judah proper except Arad), nor has any destruction layer been uncovered. Based on O. Zimhoni’s study of the pottery (2004, 4:1707), Ussishkin dated level IV to the mid-9th century B.C.E. and level V to the first half of the 9th. (This incidentally supports the LC, but Ussishkin notes that the interpretation is not conclusive.) If Lachish V dates to the 9th, Rehoboam could not have fortified the site (2 Chr 11:5–12, 23) nor would Shoshenq have destroyed it.

In the Negev there was a settlement gap throughout the LB, but a renewal in population came in Iron I. The Beersheba Valley is normally

too arid for farming, but there have been a few periods of sufficient rainfall, including the period from the 13th to the 10th centuries (Herzog 1994, 125–26). This increased fertility and peaceful conditions (apparently), and led to population growth (ca. 1000 in the 11th century). New settlements included Tel Masos, Arad, Tel Haror, Tel Sera^c, Tel Esdar, Tel Beersheba, Nahal Yattir, and other sites. Some sites (e.g. Tel Sera^c and Tel Haror) were settled by Philistines, but the identity of those moving into other sites is controversial (see the survey in Herzog 1994, 146–48). The biblical text mentions a number of groups associated with this region: Judah and related groups, Simeon, Amalekites, Canaanites, Kenites, Calebites, and so on.

The transition from Iron I to Iron IIA was marked by a significant abandonment of settlements. Tel Masos was destroyed or abandoned, but it was rebuilt as a small fortress, and a number of other fortresses were also established. It has been argued that these changes were the result of intervention by the Jerusalem monarchy, but “the spatial distribution of the sites in the valley and the demographic estimates does not support this model”; rather, a good case can be made that the cause was environmental (Herzog 1994, 143–45). Other suggestions are expanding highland settlements or Shoshenq’s invasion (Finkelstein 2003, 78–79). Certainly, the sites most uncontroversially associated with the campaign of Shoshenq are found in the Negev. The “Greater Arad” of Shoshenq is generally agreed to be Tel Arad (Finkelstein 1999a, 38–39). Arad has been much debated over the years, with later studies substantially disagreeing with the original excavation reports. Stratum XI had been identified with Shoshenq’s invasion, but Zimhoni (1985) and A. Mazar and E. Netzer (1986) concluded that XI must be later than the 10th century, which would make stratum XII the Shoshenq level. The latest excavator, Z. Herzog (2001; 2002, 58–72), agrees with this. Thus Arad XI and Beersheba V (with the first Iron Age fortifications in Judah) must be in the 9th century.

Analysis. There is wide agreement that Arad XII and related Negev sites are to be related to Shoshenq’s invasion (Mazar 2007; 2008). The use of Shoshenq’s list of cities provided a chronological anchor of crucial importance (Ussishkin 2008). Albright’s assumption that Shoshenq destroyed the whole country has been widely accepted, but his actions might have been varied: the erection of a royal stele at Megiddo shows that Shoshenq aimed to hold the city, not destroy it. Thus, his list is useless as a secure archaeological and chronological anchor, the only possible exception being Arad XII. If Arad XII dates to the second half

of the 10th century and not later, this affects the dating of Lachish V, Tel Beit Mirsim B, and Tel Beersheba VII; however, Ussishkin (2008) has some doubts about the reliability even of this synchronization.

The main advantage of the “low chronology” is that it closes the “Dark Age” of the 9th century (Finkelstein 1996a, 184–85). Its main disadvantage is that it changes the entire understanding of the emergence of the Israelite state. The monuments previously associated with the United Monarchy are redated from the second half of the 10th century to the early 9th. The “low chronology” forces reconsideration of several issues relating to the archaeology of proto-Israel. In the hill country settlements of the 10th century are not much different from those of the 11th; thus, the real transformation came about 900 B.C.E. rather than about 1000 B.C.E., which has consequences for a united monarchy. In the northern highlands this transformation brought significant growth in the number and size of sites and expansion into new frontiers and niches, while the southern highlands were only sparsely settled in early Iron II.

“Accepting the Low Chronology means stripping the United Monarchy of monumental buildings” (Finkelstein 1996a, 185). According to Finkelstein (1999b, 39), many of the strata dated to 11th century should now be the 10th (Megiddo VIA; Beth-shean Upper VI, Tel Hadar IV, Jokneam XVII, Beersheba VII, Arad XII). Important for our purposes are those redated to the 9th century according to the LC: Megiddo VA–IVB, Beersheba V, Arad XI. Those strata dated to the 8th and 7th centuries are not affected. The main mounds in both the north and the south (Megiddo, Gezer, Beersheba, Lachish) would be dated to the 9th century B.C.E. or even later. Although inscribed seals and impressions are known from mid-9th century, they are mainly found in the 8th century onwards. Finally, the LC closes the unexplained gap between the monumental architecture traditionally assigned to the 10th century and the evidence of public administration for which we have clues in the late 9th to the 8th. In sum, the 10th century is closer to the previous period than to the Iron II; the real revolution came in the 9th century, more in the north than the south. The line between Iron I and Iron II came in the early 9th century rather than about 1000 B.C.E. The kingdom of David and Solomon would have been a chiefdom or early state, but without monumental construction or advanced administration (cf. the early Ottoman Turks or Shechem in Amarna age).

Mazar’s MCC seems to have been fairly widely accepted, but its extended Iron IIA spanning both the 10th and 9th centuries means that “it would make the position of those who wish to utilize archaeology for secure historical interpretation of the 10th–9th centuries B.C.E. harder to

sustain” (Mazar 2007; cf. 2008). It also means that events that were dated to the 10th century in conventional chronology—and which the LC dates to the 9th century—are left unspecific in the MCC. Ussishkin (2008) has expressed his pessimism about resolving the issues about chronology by normal archaeological methods, referring to the

...ambiguity of clear stratigraphic evidence in many sites, and the difficulty of comparing pottery from different regions of the country. Hence there are possibilities for different interpretations and different chronologies. In my view, as long as no new additional data are available it would be impossible to solve the chronological differences being debated at present.

He goes on to refer to radiocarbon dating, a “method that has been enthusiastically adopted” by some scholars on both sides of the debate, but “this method is far from providing conclusive and perfect results,” for “the interpretation of the same ^{14}C tests can be fitted to different ideologies” (Ussishkin 2008). As quoted above, Mazar has noted the problems with radiocarbon dating but, nevertheless, refers to the results that he feels support his case. It may be that radiocarbon data will lead to a resolution or partial resolution of the debate, but this is likely to be only after a substantial database has been established (Sharon et al. 2005).

These chronological issues are occasioning considerable debate at the moment—even passionate excesses of rhetoric in some quarters—no doubt in part because they are central to the question of the United Monarchy. But rhetoric and emotion too frequently go much further than evidence, and the issue is unlikely to be resolved in the near future. Whatever one’s viewpoint, we all have to accept that the LC might turn out to be right, wholly or in part. In spite of optimistic statements about “nails in its coffin,” the LC is still a viable option in the light of present data. However, one could in theory accept the MCC or even the high chronology without accepting the United Monarchy. Unfortunately, this makes interpreting the archaeology of the Omride and Jehu dynasties fraught with uncertainties.

Another conclusion (only partially divorced from the chronology debate) is the difference between the development in northern Palestine as contrasted with southern Palestine. This was largely due to geographical factors: topography, geology, soil, rainfall, climate in general. The northern region, both the hill country and the lowlands, was much more suited to settlement, farming, and fertility in general. The region of Judah was less fertile, had a lower density of population, and developed economically more slowly than Samaria. When one considers the *longue durée*, it would have been extraordinary for the Judaean highlands to

dominate the north in the Iron I or IIA. A number of archaeologists argue that the archaeology does not support the text which depicts a Judaean highland-centred United Monarchy. Those who do argue for archaeological support for the United Monarchy generally do so by explicit—or implicit—appeal to the text as the guide for interpreting the archaeology.

Jerusalem remains an area of considerable controversy, but those who maintain that Jerusalem did not develop into a substantial city until Iron IIB have current archaeology on their side (though the building recently found by E. Mazar has intriguing possibilities). Those who maintain an earlier development must argue on the basis of what was presumed to have disappeared or what might be found in the future. This is why a substantial argument is now made that the northern kingdom (in the form of the Omride dynasty) was the prior development to a state in the 9th century, with Judah coming along more slowly, reaching its height only in the 8th century. But the debate continues.

Medinet Habu and Related Inscriptions

An inscription in the name of Merneptah, dating to his 5th year (conventionally dated to 1207 B.C.E., though Kitchen insists it is 1209/1208), has the first reference to Israel known and the only reference until the 9th century. Accompanying it are reliefs from the Karnak temple that have been associated with some of the events described in the poem (on these, see below). Most of the inscription is about Merneptah's defeating the Libyans who attempted to conquer Egypt, along with help from a revolt in Nubia. It is only right at the end that statements are made about other peoples supposedly conquered by Merneptah, one of which seems to be Israel (translation of Redford [1986, 197]; textual quote from Niccacci 1997, 64):

Tjehenu is seized, Khatte is pacified,
Pekana'an (Gaza) is plundered most grievously
Ashkelon is brought in and Gezer captured,
Yeno'am is turned into something annihilated,
Israel is stripped bare, wholly lacking seed!
[*Ysr3r fk(w) bn prt.f*]
Kharu has become a widow for Egypt
And all lands are together at peace.

Although the reading “Israel” (for Egyptian *Ysr3r*) has been widely accepted, not everyone agrees. For example, the name *Ysr3r* has been read as “Jezreel,” as well as some less credible renderings (Eissfeldt 1965; Margalith 1990, 228–30; Hasel 1998, 195–98; Hjelm and Thompson 2002, 13–16). From a philological point of view, this seems an

unlikely reading, as do some of the other suggestions (Hasel 1998, 197–98; Kitchen 2004, 270–71). All in all, it seems that the reference to Israel is reasonably secure. Much debate has centred around the determinative (cf. Yurco 1986, 190 n. 3; Hasel 1998, 198–99). The other three names have the three-hills and throw-stick signs, which are normally used for a foreign territory, whereas Israel has a seated man and woman with the throw-stick, which suggests a people rather than a fixed geographical site. These data have been used in arguments about Israel’s origins. Another question concerns the phrase “his seed is not” (*bn prt.f*). It has often been taken metaphorically to refer to “descendents, offspring” (e.g. Niccacci 1997, 92–93), but recently it has been argued that this means “grain,” suggesting that Israel is a sedentary community of agriculturists at this time (Hasel 1998, 201–3). Rainey (2001) argues strongly that it should be understood as “descendents,” though this translation is taken as evidence for his own interpretation of how Israel originated.

The question is: Is this inscription only a piece of royal propaganda—a triumph-hymn—with little or no historical value (cf. Hjelm and Thompson 2002)? It is one of four sorts of royal inscription and includes extravagant praise of the king as a matter of course, but this by itself does not resolve the matter because factual material is also included at relevant points in such inscriptions (Kitchen 2004, 260–65). The argument—really, more of an assertion—that Israel is only an eponym (“analogous to Genesis’ Israel: the patriarch of all Palestine’s peoples”) ignores the determinative, which is plural and which refers to a people. According to Kitchen (2004, 271) the oft-made statement that a number of errors in determinatives are found in the inscription is incorrect. As for Israel’s being paired with Kharu, this is only one possible analysis. In fact, a number of different literary structures have been seen in the passage (summarized in Hasel 1998, 257–71). There is also the question of whether *Pekana'an* refers to “Canaan” or “Gaza.” In spite of Hjelm and Thompson, the conclusion that this inscription “has been considered correctly as concrete proof of an Israel in Palestine around 1200 B.C.E.” (Lemche 1998, 75) remains the most reasonable one.

More controversial are the reliefs (Redford 1986; Yurco 1986; 1997, 28–42; Hasel 1998, 199–201). The reliefs in question give ten different scenes: the first four are the main ones, which picture the Pharaoh triumphant in battle; the fifth pictures bound Shasu prisoners; and the sixth shows Canaanite captives being led to a chariot. Redford (1986) has argued that the inscriptions originally related to Rameses II and have been altered to fit later rulers, and there is no reason to associate all of them with Merneptah, who was in poor health and decrepit when he

came to the throne. According to Redford, there “is absolutely no evidence that Merneptah attacked all these places during his short reign. To the best of our knowledge, during his rule there occurred no triumph over Khatte...nor any defeat of Gaza or Yano‘am” (Redford 1986, 197).

F. Yurco has argued (against Redford) that the first four reliefs can be equated with the four names in the inscription (Yurco 1986; 1997, 28–42). In other words, scene 1 describes the conquest of Ashkelon; scene 2, of Gezer; scene 3, of Yano‘am; and scene 4, of Israel. He concludes that the scenes pictured agree with the determinative that accompanies each name, with the first three shown as cities and the fourth (Israel) as a people but no city. Yurco’s argument that the reliefs are to be ascribed to Merneptah seems to have won over some (cf. Kitchen 2004, 268–70)—though Redford seems to maintain his position (1992, 275 n. 85). But the equation of the reliefs with the four names in the inscription is rather less secure (Rainey 2001, 68–74). In only the first scene is the site named (Ashkelon), but no names are found in scenes 2–4. Also, it may be that there were once other scenes on the wall that are now missing because of deterioration of the structure. Thus, the relating of specific names to specific scenes is much more hypothetical than Yurco seems to allow.

The interpretation of the reliefs is important to the various antagonists primarily because of the identity of the peoples being defeated. Yurco’s main concern seems to focus on the dress of those fighting the Egyptians. He argues that the defeated Israelites have the typical dress of the Canaanites, providing evidence that Israel was like (and thus arose from) the Canaanites. Rainey (2001, 72–74) argues that, on the contrary, the Israelites are to be identified with another group who are pictured in scene 5, the Shasu (also Redford 1986, 199–200). Although this seems to be a definite possibility, his arguments for a positive identification seem to be no stronger than those of Yurco for the Canaanites.

Finally, there is the question of where Israel is supposed to reside. A number have asserted that it refers to the hill country (e.g. Dever 2001, 118–19). Kitchen (2004) argues that each name refers to a section of Palestine: Ashkelon to the coast, Gezer to the inland area, Yano‘am to the Galilee; therefore, Israel would refer to the hill country. This is far from cogent. There is nothing in the inscription to suggest that the individual names were meant to refer to a specific part of the country—Merneptah may just be listing sites and peoples conquered. Also, the sections of Palestine listed for the first three names by Kitchen do not cover all the territory except the hill country: what of the Valley of Jezreel, the Jordan Valley, the Negev, the Transjordanian region, the plain of Sharon, and so on? N. Na’aman (1994, 247–49) points out that it

is possible that the author mentioned the cities first and then the people, so there was no sequential listing. The conjectured location is highly speculative: some put “Israel” in the area of Shechem, but the Egyptians called it “the land of Shechem” or “the mountain of Shechem”; putting Israel in Manasseh is nothing more than guesswork. In conclusion, it is “best to refrain from building on this isolated reference any hypothesis concerning the location and formulation of Israel at that time” (1994, 249). Thus, no argument has so far been presented that pins down the exact location in the land of this entity Israel. Ultimately, the only thing we can say is that the inscription proves there was an entity called “Israel” in Palestine about 1200 B.C.E. This is an important datum, but it does not allow us to be certain of where it was located (if indeed there was a single location) or the precise organization or status of this entity “Israel.”

Rameses III’s temple at Medinet Habu is very important for reliefs and inscriptions that relate to the invasion and defeat of the Sea Peoples (*ANET*, 262–66; *ARE* vol. 4, §§59–82; O’Connor 2000; Wachsmann 2000), along with a few other sources of lesser importance (Redford 2000, 8). These are not straightforward historical accounts but require a fully critical approach; nevertheless, with a careful reading much can be learned (Redford 2000; see also pp. 94–99, below). The following excerpt is taken from *ANET* (262, italics and square brackets in the original):

(16) ...The foreign countries made a *conspiracy* in their islands. All at once the lands were removed and scattered in the fray. No land could stand before their arms, from Hatti, Kode, Carchemish, Arzawa, and Alashiya on, being cut off at [one time]. A camp [was set up] in one place in Amor. They desolated its people, and its land was like that which has never come into being. They were coming forward toward Egypt, while the flame was prepared before them. Their confederation was the Philistines, Tjeker, Shekelesh, Denye(n), and Weshesh, lands united. They laid their hands upon the lands as far as the circuit of the earth... I organized my frontier in Djahi, prepared before them:—princes, commanders of garrisons, (20) and *maryanu*. I have the river-mouths prepared like a strong wall, with warships, galleys and coasters...

Report of Wenamun

One interesting text is the *Report of Wenamun*, about a journey to the Phoenician and Palestinian area (*AEL* vol. 2, 224–30). The text is usually dated to the early 11th century B.C.E., allegedly before Egypt lost its hold on the region. It appears to give useful insights into the relationship between Egypt and the Phoenician area and contains many useful incidental details.

Shoshenq I's Palestinian Inscription

According to 1 Kgs 14:25–28 a king Shishak of Egypt came up against Jerusalem in Rehoboam's 5th year and took all the treasures of the temple. When an inscription of Shoshenq I (ca. 946/945–925/924) was found at Karnak listing many topographical sites in Palestine, a connection was made with the passage in the Bible and has been the standard view ever since. All seem to agree that Shoshenq's expedition was a signal event in Israel's history, but precisely what happened on the ground and even when the invasion took place is considerably disputed. The conventional view is heavily informed by the Bible. According to it, Shoshenq's army made a number of destructive raids on various parts of Palestine, destroying many sites in the Negev and even as far north as Megiddo; however, Jerusalem did not fall because the Pharaoh was bought off by Rehoboam.

A number of studies have addressed the issue of Israel/Judah and Shoshenq's "invasion," beginning with M. Noth's study in 1938 (for a survey of earlier studies, see Wilson 2005; also Schipper 1999, 119–32; Ash 1999, 50–56). Most have assumed that Shoshenq conducted an invasion of Palestine, that the inscription gives some sort of invasion route, that the inscription can be reconciled with the biblical text, and that the archaeology matches the inscription. There have, nevertheless, been some problems, especially the fact that Israel and Judah are not mentioned specifically, that no site in Judah occurs in the inscription, that the toponyms cannot be worked into any sort of itinerary sequence, and that the biblical text says nothing about an invasion of the Northern Kingdom.

It might not be surprising if a Jerusalem scribe did not record the details of Shoshenq's raids on Israel, but why omit the destructive attacks on the Negev, which was a part of Judah—at least, in the eyes of the Bible? Leaving aside the Jerusalem question, there is still considerable disagreement about how to interpret Shoshenq's inscription. Was it a raid or primarily an occupation—albeit temporary—of the land? Various explanations have been given of the order of toponyms as they might relate to the progress of the invasion, but none has been completely convincing. Now, however, K. Wilson has investigated Shoshenq's inscription in the context of other Egyptian triumphal inscriptions. He concludes:

- Triumphal inscriptions were designed to extol the Pharaoh's exploits, not provide historical data.
- The reliefs glorify all the exploits of the king rather than a particular campaign.

- The topographical lists are not laid out according to any system that allows a reconstruction of the military route.
- The sites listed may in some cases be those attacked, but others not attacked—indeed, friendly towns and allies—might be listed as well.
- The lists were apparently drawn in part from military records and onomastical lists, which means that some data of value for certain purposes may be included.

The implications of these conclusions are considerable. Rather than recording a particular campaign into Palestine, Shoshenq's inscription may include more than one (as maintained by Knauf 2007). This would help to explain the vague nature of the inscriptions that accompany the topographical lists, without clarifying the reasons for or objectives of the "invasion." In any case, the precise nature and progress of the campaign(s) cannot be worked out. More puzzling is the lack of any reference to Judah or Jerusalem as such. The argument is that this was in a section of the inscription that is no longer readable. This argument is still maintained by the latest study of the Shoshenq inscription by Wilson (2005). It must be said that this argument, while possible, is not compelling. Another obvious interpretation is that Shoshenq bypassed Judah—or at least, the Judaean highlands—because it did not suit his purpose, and the biblical writer got it wrong. Interestingly, the solution that seems to be agreed on by both A. Mazar and I. Finkelstein (both in Grabbe, ed., 2008) is that Shoshenq was indeed interested in coming up against Judah because of the copper trade. This could make Jerusalem not just a stage in the invasion but its main object (though not Finkelstein's view). This is an interesting interpretation, though one might ask why Shoshenq then pushed on north as far as the Jezreel Valley if he had already reached his objective.

Finkelstein, among others, argues that the main phase of prosperity was post-Shoshenq and that the sites in the south were primarily not destroyed but abandoned. They point out that Shoshenq also doesn't mention the Philistine cities, which could be significant. He interprets it as evidence for their control of the copper trade. But whether or not that is right, we have to ask why the Philistines were omitted. If the Egyptian expedition was a general attack on Palestinian cities in Israel and Judah, why should the Philistine plain be omitted? Could these cities have a particular relationship with Egypt? Or was the Shoshenq operation a more complex one? David Ussishkin (2008) makes the reasonable argument that Shoshenq would hardly set up his stele in a ruined city, but

suggests that Megiddo was not just attacked but was occupied to become a regional headquarters. To me, this calls for a rethink of how destructive Shosheng's raid was, as opposed to dominance and intimidation.

Tel Dan Inscription

Perhaps one of the most interesting texts is the one recently found at Tel Dan, which already has a considerable bibliography (Athas 2003; Kottsieper 2007). The following is my reading of the first fragment found in 1993 (Biran and Naveh 1993) with a minimum of reconstruction:

] my father went up [
my father lay down (died?). He went to [Is-]
rael earlier in the land. My father [(or “in the land of my father”)
I—Hadad went before me [
x my king. And I killed of [them chari-]
ot and thousands (or 2000) of riders [
king of Israel. And I kill[ed
xx “house of David” (*bytdwd*). And I set [
xx the land. They x[
another, and xxxx [ki-
ng over Is[rael
siege over [

The second fragment (actually two fragments that fit together) does not clearly join onto the first, and the reconstruction based on putting the two together strikes me as purely speculative (cf. Athas 2003, 175–91). I read the second fragment as follows, with little hypothetical reconstruction:

] and cut [
] battle/fought against xx [
]x and went up the king x [
] and Hadad made king [
] I went up from Sheva/seven [
seven]ty tied/harnessed x[
]rm son [
]yahu son [

This inscription has been subject to a number of interpretations, some of which are quite compelling, but they rely generally on the reconstruction of the original editors. However, it does seem to me that, in the last two lines above, the restoration of “Jehoram/Joram” is virtually certain, and of “Ahaziah” quite reasonable. If so, this favours assigning the inscription to Hazael and the interpretations that follow from it, which would put it shortly after 840 B.C.E. Finally, there have been some isolated doubts about the authenticity of one or more of the fragments, but these have not been convincing (Athas 2003, 22–35, 70–72).

Biblical Text

Most of the texts of relevance to this time period are from the Pentateuch or the Deuteronomistic History (DtrH). Both of these are collections that scholarly consensus regards as late compilations. The Pentateuch seems to have been put together in the Persian period (cf. Grabbe 2004, 331–43; 2006b), though many date Deuteronomy to the 7th century B.C.E. For DtrH most specialists choose between an ascription to either a two-fold composition, the first edition in the late 7th century and the second in the exilic period, and a single composition in the 6th century (O’Brien 1989; Campbell and O’Brien 2000). In each case, the compiler(s) would have used a variety of traditions, as well as making their own edits and additions. Yet many see it completed later in the post-exilic period. The main texts relevant for Israel’s settlement in the land are the books of Exodus, Numbers, and Joshua and Judges.

Exodus. The main incident in the book of Exodus is the enslavement of the descendants of Jacob and then their miraculous liberation through the power of Yhwh. Whatever the reality, it is clothed in a thick layer of mythical interpretation. The Pharaoh is a generic figure, without a name. A series of ten miracles is enacted—and attempts to find naturalistic explanations (e.g. Hort 1957) miss the point: the aim of the narrative is to magnify the power of Yhwh and his servant Moses. According to the plain statement of the text, 600,000 men of military age came out; with the elderly, women, and children, the number would have been at least three or four million (Grabbe 2000). The crossing of the Red Sea seems to mix a more naturalistic account, in which an east wind moves the waters (Exod 14:21), with a more miraculous one in which the sea divides and the water stands on either side like walls (Exod 14:22–29). F. M. Cross (1973, 121–44) attempted to argue that a naturalistic account, in which the Egyptians died in a storm as they pursued the Israelites across the sea in boats, is reflected in Exod 15:7, but his philological analysis is flawed (Grabbe 1993). Thus, even if Exod 15 is an example of early poetry, as some argue (e.g. Robertson 1972; Cross and Freedman 1955; Cross 1973, 121–44), it does not appear to give a picture different from the surrounding narrative (for the argument that Exod 15 is not early, see Noth 1962).

Numbers. Israel’s journeyings for the 40 years after the revelations on Sinai are only sketchily laid out, but Num 33 is a key passage. One of the main incidents in the Numbers account is the conquest of the Transjordanian region. Trying to extract historical data from this tradition is difficult. There is some indication, for example, that at least some of the

peoples listed in the tradition are simply creations from the names of mythical figures. Some of the most feared inhabitants of the land are the Anakim who are descended from the Rephaim (Num 13:35; cf. Deut 1:28; 2:10, 11, 21; 9:2; Josh 11:21–22; 14:12, 15; 15:14). One of the main figures is Og of Bashan. He is said to be from the remnant of the Rephaim and dwells in Ashtarot and Edrei (Num 21:33–35; Deut 1:4; 3:10–11; Josh 9:10; 12:4; 13:12; cf. Num 13:33). These names are significant. Other passages (such as Job 26:5; Ps 88:11–13; Isa 26:14, 19; Prov 9:18), as well as the Ugaritic texts, associate the Rephaim with the dead. The god Rapha'u of a Ugaritic incantation seems to dwell in Ashtarot and Edrei (*KTU* 1.108). Thus, it appears that myth has been historicized, and the shades of the dead have been turned into ethnographical entities. The writer seems at times to have taken traditional or mythical names and used them to create a narrative about ethnic groups. There is also some evidence that the writer has drawn on topographical knowledge of the 8th or 7th centuries to draw up his list of journeys (see further below under “Question of the Exodus”).

Joshua. The book of Joshua describes how the Israelites crossed the Jordan (Josh 1–5) and conquered the land in only five years, the major cities taken being Jericho, Ai, while Gibeon and Kiriath-jearim submitted without a fight (Josh 9). Some decades ago the broad picture of the conquest of Canaan by the Israelites was widely accepted in parts of scholarship, especially in North America, where the “Albright hypothesis” was very influential (see below, “The Question of Settlement”). The surprising thing is that when the reader moves from Joshua—where the land was conquered and divided up—to Judges, there is something of a shock, since the land seems far from Israelite control. Granted, here and there are statements in Joshua that suggest that everything was not conquered all at once (e.g. Josh 17:14–17), which means that something of a mixed message comes across. Nevertheless, the dominant impression is that it was all settled after five years. This is especially the impression left by Josh 11:23; 14:15; 21:41–43, and Josh 13–19 where the land is divided up among the various tribes. The “Albright” or “Conquest” hypothesis is generally not accepted anymore, and the question to be asked is whether any of the book of Joshua can be taken as historical. Many would now answer this in the negative (e.g. Na’aman 1994, 249–81; Van Seters 1990; Miller 1977). Some lists seem to relate to a much later time and situation (cf. Na’aman 2005 on Josh 15:21–62; 18:21–28; 19:2–8, 40–46). There seem to be a few passages that remember data from the distant past, but these are very few (see E. A. Knauf [pp. 130–39, below]).

Judges. As noted in the previous section, if one had read Joshua for the first time and then moved to Judges without knowing anything about its contents, it would produce considerable consternation, because a number of the things supposedly accomplished in Joshua have to be done again (e.g. Judg 1:19–36 vs. Josh 16–19). The book of Judges is mainly made up of a series of episodes which follow a common pattern: Israel sins, is punished by being made subjects of a foreign people, cries to Yhwh, has a deliverer sent who leads them in throwing off the foreign yoke—following a 40/80 year cycle. This structure is clear through much of the book. These stories, in addition to their entertainment value, had an important morality content, which is probably the main reason they were told. But our concern is with their historicity.

The general picture of Judges has often been seen as an authentic representation of pre-state Israelite society, and this may indeed be the case. As has long been recognized, the narrative of Judges is divided between heroic deliverers (“major judges”) and civic leaders (“minor judges”). The latter get little actual space but are presented in two brief lists (Judg 10:1–5; 12:7–15). Shamgar ben Anath has a curious name, because it suggests a worshiper of the goddess Anat (3:31; 5:6). The judge named Jerubbaal is only later identified with Gideon (chs. 6–8, with 6:32 giving a nonsensical etymology of his name; it means something like “Let Baal be great”). Abimelech (son of Gideon) looks like the king of a Canaanite city-state (ch. 9). Jephthah occurs in the list of “minor judges” but also appears as a heroic figure, whereas Samson seems independent of the other deliverer stories. One cannot rule out that some actual historical core can be found in the “deliverer” stories, but when we turn the statement around, demonstrating such a core of history is very difficult. B. Halpern (1988) argued for such a core of historicity in the Ehud/Eglon story, but E. A. Knauf (1991) has cast considerable doubt on his argument by showing that “Ehud” was a Benjaminite clan, and Eglon, a town in the Judaean foothills. Despite references to “all Israel” only one or two tribes are normally involved in the action in an episode; the Song of Deborah (Judg 5) is the only passage with more than two tribes.

It has often been argued that the “Song of Deborah” is an example of early poetry (Robertson 1972), which would suggest that it was written close to the events described (Stager 1988; see the summary of arguments in Lindars 1995, 212–17). The list of Israelite tribes differs in several ways from all other lists (Machir instead of Manasseh [5:14]; Gilead instead of Gad [5:17]; Judah, Simeon, Levi are absent), which might demonstrate an independent tradition and one possibly earlier than

other traditions. Nevertheless, a number of scholars have argued that the Song of Deborah shows signs of lateness (e.g. Diebner 1995; Lindars 1995, 213–15), and assuming that it is more trustworthy as a historical source is misplaced confidence. P. D. Guest (1998) has argued that far from being a compilation of different sources, the book of Judges shows the marks of unitary authorship that produced a “crafted history” of the period: “Although the text presents itself as history, it should not be mistaken for such” (1998, 61). In one area, however, Judges may reflect an older linguistic usage: the title “judge” (Hebrew **שֹׁפֵט**). Although the word means “judge” in a judicial sense in most Hebrew usage, the reference to an individual in Judges means something like “political/military leader” (Niehr 1994). Thus, here and there may be reliable early traditions (see those catalogued by E. A. Knauf [pp. 140–49, below]), but demonstrating them is difficult; the book is generally too problematic to use as a historical source.

The Saul and David Stories (1 Sam 13 to 2 Kgs 2:10). These traditions are difficult to summarize without getting into questions of sources and credibility. It seems best to keep all the discussion together. Accordingly, we move on to analysis.

II. Analysis

Albrecht Alt set the example for all those who study the question of the Israelite settlement. He showed the importance of understanding the geographical, environmental, and historical context of what was happening in Palestine through this period, including the international political context. Politically, the entirety of Palestine remained under the control of Egypt (at least, nominally) until about 1130 B.C.E. This is very important to be aware of. The inhabitants of Palestine were not in a position to pursue whatever course of action they chose, especially if it was likely to conflict with the interests of Egypt. With the defeat of the Sea Peoples and their settlement in the coastal plain, any settlements or expansions of territory in the central part of the country would have been constrained in expansion to the west. The Shephelah between the coastal plain and the hill country became the border zone that was fought over by the inhabitants of the coastal religion and those of the hill country.

The Question of the Exodus

Some still invoke the exodus tradition to explain Israel in the land, either because it is believed that the ancestors of some of those who made up Israel had spent time in Egypt or because—a view now almost entirely

rejected by mainstream scholarship—Israel had migrated as a people from Egypt and taken over the land of Canaan. Before talking about the rise of Israel in the land, let us consider the most recent scholarship relating to the exodus.

The idea that the ancestors of Israel were in Egypt for a period, that they were oppressed, that they came out of Egypt “with a high hand,” and that they entered the Promised Land after a period in the wilderness is a major concept in the biblical text. How historical is this? The last part of the picture—the conquest of the land of Canaan—will be discussed below, but here we focus on the general question of the exodus. Several issues are involved:

1. The exodus tradition in the biblical text. The vast bulk of the Pentateuchal text describing the exodus and related events seems to be quite late (Albertz 1994, 23–24, 42–45, “exilic or early post-exilic”). The question is whether the exodus is presupposed in early texts. It was once widely argued that the exodus was embodied in certain passages quoting an early Israelite “credo” (von Rad 1965), but subsequent study suggested that some of these passages (e.g. Deut 6:21–23; 26:5–9; Josh 24:2–13) were actually late (Nicholson 1973, especially 20–27). Some point out that Hosea (12:1; 13:4), for example, presupposes the exodus tradition. Not everyone is confident any longer in such literary analysis; in any case, this would take us back only to the 8th century, long after the alleged event. The biblical text does not provide any particular time for Israel’s coming out of Egypt, and a number of the dates assigned to the event depend on data not really relating to the exodus itself (e.g. the settlement, the Merneptah stele).

2. Semites in Egypt. A number of Egyptian texts from the second millennium B.C.E. mention peoples who were non-Egyptian and probably Semitic (see the survey in Malamat 1997). The Egyptian texts refer to “foreigners” under the categories of “Asiatics” (*ȝmw*), Nubians, and Libyans (Leahy 2001, 548). None of the “Asiatics” mentioned in Egyptian texts is referred to in such a way as to make one identify them with Israelites (though see the discussion of the Shasu above under “Medinet Habu and Related Inscriptions”). What it does indicate is that the idea of people from Syro-Palestine—including possibly some early Israelites—living for a time in Egypt is not in and of itself problematic.

3. The Merneptah stele (see above under “Medinet Habu and Related Inscriptions”). Appeals to this text as evidence for the exodus are very problematic. The inscription provides no evidence for any sojourn in Egypt for those identified in the text as “Israel”; on the contrary, this entity appears to be in Palestine (though Knauf [pp. 241–50, below] suggests that Israelite prisoners of war were taken to Egypt for a time).

4. References in Egyptian texts (Frerichs and Lesko 1997; Davies 2004). There is nothing in Egyptian texts that could be related to the story in the book of Exodus. It is not just a question of the official ignoring of defeats of the Pharaoh and his army. There is no period in the second half of the second millennium B.C.E. when Egypt was subject to a series of plagues, death of children, physical disruption of the country, and the loss of huge numbers of its inhabitants. Occasionally, a scholar has seen a remarkable resemblance between Moses and some Egyptian official, but the arguments have not met widespread acceptance. At most, one could say that a memory of the Egyptian figure was used to create the figure of Moses in the biblical text. G. I. Davies (2004) surveys about all the evidence available, but little of it is very compelling; indeed, he finds the attempts to equate the exodus tradition with certain figures known from Egyptian sources as “not very convincing.” Thus, his conclusion that the exodus “tradition is *a priori* unlikely to have been invented” appears tacked on rather than arising from his data.

5. Egyptological elements in the exodus narrative. Some have argued that elements within the text fit the period of Rameses II (Hoffmeier 1997), but this is not sufficient; one must show that they do not fit any other period in history (see nos. 6 and 7 below). It has been widely accepted that there are names and other references that suggest some knowledge of Egypt in the exodus narrative, but how early are they? What is notable is that there are few incidental or accidental references to Egypt, such as one might expect, unlike some other biblical passages such as found in Isaiah and Jeremiah; most of what is present is topographical (Redford 1987, 138). More important, a number of the Egyptian elements in the exodus story are anachronistic, for example, reference to the “land of Goshen,” which occurs only in later Egyptian texts (Redford 1987, 138–49). There is no agreement among Egyptologists about elements that could only be dated to an early period.

6. The cities Raameses (Pi-Ramesse) and Pithom (Exod 1:11). The first problem is identification of the sites in question. Pi-Ramesse is widely identified with Qantir (Pusch 2001; Bietak 1987, 164; Hoffmeier 1997, 117), though Redford asked where the “Pi” (Egyptian *pr*, “house”) of Pi-Ramasse had gone (1963, 408–10; 1987, 138–39; but cf. Helck 1965). No agreement about the identity of Pithom (Egyptian *pr-‘Itm*, “the house of Atum”) has been reached (Wei 1992). Many argue for Tell el-Maskhuta (Redford 1963, 403–8; Holladay 2001), but this site was not settled between the 16th and the 7th centuries. The nearby site of Tell el-Ratabah is another possibility, but it was reoccupied only about 1200 B.C.E. (Wei 1992; Dever 1997, 70–71). It is therefore difficult to

understand Davies’ statement, “they are more likely as a pair to belong to a tradition that originated in the Ramesside period than to a later time” (2004, 30). If Tell el-Maskhuta was known as “Pithom” from about 600 B.C.E. (Davies 2004, 30) and topographical names with “Rameses” were also widespread in the first millennium B.C.E. (Redford 1987, 139), this argues that the tradition of Exod 1:11 was likely to be late, rather than Ramesside.

7. The supposed route of the exodus from Egypt. Some have argued that the route of the Israelites’ journeyings in the Bible matches the actual topography and Egyptian settlements on the ground (Hoffmeier 2005; Krahmalkov 1994). A more careful look shows, however, that the text does not reflect the 15th or 13th century B.C.E. but the 7th or 8th (MacDonald 2000, 63–100; Dever 2003, 18–20). Some of the itineraries are rather vague, showing little actual knowledge of the topography supposedly being described (Deut 2:1–25; Num 21:10–20), with Num 33:1–49 going the farthest in suggesting knowledge of a real travel route (MacDonald 2000, 98). Only at the end of Iron II (but not Iron I or early Iron II) were most of the sites that can be identified actually occupied. Most scholars argue that the itineraries in Exodus and in Num 33 are the result of late editing of several different traditions that do not presuppose the same route (Haran 1976; Davies 1990).

8. Conquest of the Transjordanian region. On this point, see the comments above under “Numbers” in “I. Sources.”

9. Archaeological evidence. No event of the size and extent of the exodus could have failed to leave significant archaeological remains. Israel’s itinerary has already been discussed (point 7 above). According to the book of Numbers (10:11; 12:16; 13:26; 20:1, 22; 33:36) much of the 40 years of “wandering” was spent near Qadesh-barnea. This and related sites in Sinai and southern Palestine should yield ample evidence of a large population in this region. Yet we find nothing (Finkelstein and Silberman 2001, 62–64). Qadesh (Tell el-Qudeirat) itself has been extensively excavated but shows no habitation between the Middle Bronze and the 10th century B.C.E. (Cohen 1981; 1997; Dever 2003, 19–20) or even later (Ussishkin 1995).

Opinions about the historicity of the exodus are divided. Despite the efforts of some fundamentalist arguments, there is no way to salvage the biblical text as a description of a historical event. A large population of Israelites, living in their own section of the country, did not march out of an Egypt devastated by various plagues and despoiled of its wealth and spend 40 years in the wilderness before conquering the Canaanites. This does not rule out the possibility that the text contains a distant—and

distorted—memory of an actual event. Some feel that the tradition is so strong in the Bible that some actual event must lie behind it, though it might be only a small group of (slave?) escapees fleeing Egypt (a view long and widely held). This is accepted even by some of those proposing theories about the indigenous origin of Israel in Canaan (see below, under “Question of the Settlement”). Some think it might even be a hazy remembrance of the Hyksos expulsion from Egypt in the 16th century B.C.E. (e.g. the Egyptologist A. Gardiner [1933]).

Yet others point out that there is no necessity for assuming there was an exodus in the early history of Israel (Dever 2003, 7–21). There is no external evidence for such an event, and any arguments must depend on the biblical tradition; however, since we know of many Egyptian connections with Israel and Judah at later times, from the time of the monarchy to the Persian and Hellenistic periods (cf. Isa 19:19–25; Jer 42–44), this could have been sufficient to give rise to the story in the biblical text. Many scholars now agree that there is little clear evidence that the biblical tradition is an early one.

The Sea Peoples and the Philistines

According to the biblical text the Philistines were the main enemy of Israel through much of its early history. This picture is manifestly wrong in certain parts; for example, the Philistines have been anachronistically projected back as settled in the land in the early second millennium B.C.E. (Gen 21:22–34; 26:1–18; Finkelstein [2002, 152–54] suggests that this reflects an 8th- or 7th-century context). Yet other parts of the biblical picture may be correct; we can know only after looking at it carefully. We know about the Sea Peoples from a number of sources (see the survey in Noort 1994; also Oren, ed., 2000; Ehrlich 1996, 1–21). The main sources for the Philistines are the Egyptian texts, primarily the Medinet Habu reliefs and inscriptions (see above under “I. Sources”), and the material culture. About these, however, there is considerable debate.

As C. Ehrlich (1996, 9–13) indicates, there are two main interpretations (depending heavily on the Medinet Habu inscriptions and other documents but also bringing archaeological data into the question). The “maximalist” interpretation argues that Rameses III fought a coalition of Sea Peoples (Peleset [Philistines], Sherden, Tjeker, Shekelesh, Denyen, Tresh, and Weshesh in both sea and land battles, defeated them, and forced (or allowed) them to settle in the Palestinian coastal plain. This would have been about 1175 B.C.E. The “minimalist” interpretation argues that the nature of Pharaonic inscriptions needs to be taken into account. Rameses III’s claims are conventional propaganda, either

created from a long literary tradition or a compilation building up a minor episode into an earth-shaking threat to Egypt from which the divine Pharaoh delivered her, as was his duty and function. (It should be noted that at least two of Rameses III’s campaigns celebrated in Medinet Habu, his victories over the Nubians and Asiatics, are completely made up [Noort 1994, 108].) The “invasion” of the Sea Peoples was the settlement (perhaps over a lengthy period of time) of a relatively small number of civilians who perhaps arrived as a result of trade links.

Although there are many problems of interpretation, several points about the Sea Peoples emerge with a reasonable probability from the reliefs and inscriptions (cf. Redford 2000, 12–13):

1. They come from “islands in the midst of the sea,” which meant Crete and the Aegean archipelago to the Egyptians (cf. the biblical “Caphtor” [Amos 9:7; Jer 47:4], which seems to mean the same thing [Drews 1998]).
2. They seem to have been well organized and well led (though this does not appear in the reliefs which, by their very nature, picture defeat and chaos). Five (sometimes more) tribes are listed, of which the “Philistines” (*prst*) are only one (see the list above).
3. The presence not just of warriors (the only ones pictured in the sea battle) but also of families and livestock, with household goods in ox carts, suggests the migration of peoples rather than just an army of conquest.
4. A sea battle seems to have taken place in the region of the Nile delta (as *r-h3 wt* “river mouths” implies), but the land battle is more of a problem. Some (e.g. Redford 2000, 13) think the land battle was also in northern Egypt, but the inscription refers to “Djahy,” which can refer to the Phoenician coast. Three permutations concerning the relationship of the land and sea battles are possible (Ehrlich 1996, 9 n. 45), but the main question relates to the land battle. Ussishkin (2008) argues that since Egypt controlled Megiddo, the land part of the movement would have been stopped in northern Palestine. Ussishkin’s view is supported in some ways by the recent book by E. Morris (2005, 697 n. 16) who comments in a footnote:

There is nothing in either the reliefs or the texts that supports the supposition of Stadelmann..., Sandars..., and Bietak...that the land battle had occurred near the mouth of the Pelusiac branch of the Nile. Indeed, Rameses III’s referral of the matter to vassals, garrison troops, and maryannu-warriors would argue strongly *against* an Egyptian locale.

The strength of the 20th Egyptian Dynasty is attested by Morris (2005, 703–5) who notes the number of Egyptian fortresses and sites.

This raises a number of questions: Did they arrive to settle primarily by ship or ox cart? If they were opposed by the Egyptians in a crucial fight in the Delta region, what about the Egyptian garrisons and general Egyptian control of Palestine to the north? There was apparently a battle at sea, but was there one on land in the same general area—or was the land battle far removed—up north of Megiddo? What material culture is to be associated with the Philistines, and in what periods?

The main indication of the Sea Peoples/Philistines in the coastal plain of Palestine is the material culture. It has been argued that the first phase of locally produced Mycenaean IIIC:1 pottery (sometimes referred to as “Philistine Monochrome”) is to be dated to Rameses III’s early reign or the second quarter of the 12th century, with no direct relationship to Mycenaean IIIA–B imports from the LB (Dothan and Zukerman 2004, 43–45). Thus, it cannot be interpreted as an “import substitution” for wares from LB Aegean and Cyprus, and “the abundance of table and kitchen wares in this assemblage demonstrates that Mycenaean IIIC:1 pottery was produced for internal consumption by the population accustomed to Aegean/east Mediterranean culinary practices” (Dothan and Zukerman 2004, 45). It is “totally different” from the local “Canaanite” pottery, leading to the conclusion that “the most reasonable historical background for this phenomenon is the establishment of the new Philistine urban settlement.” Finally, it “can be suggested that Philistia, Cyprus, and the Aegean maintained close contacts in the first half of the 12th century B.C.E.” (2004, 46).

The recent studies of the pottery by A. E. Killebrew (2008; cf. 2000) tell a somewhat different story. At Tell Miqne-Ekron stratum VII shows the sudden appearance of quantities of locally produced Aegean-inspired Mycenaean IIIC Early and associated assemblages. In stratum VI Mycenaean IIIC and Bichrome appear together throughout, showing the development from Mycenaean IIIC to Bichrome. Finally, in stratum V Mycenaean IIIC disappears and Bichrome becomes the predominant decorated ware. One theory, the high chronology (or two-wave theory), would date the Mycenaean IIIC Early (Mycenaean IIIC:1b) to about 1200 B.C.E., the result of an early proto-Philistine wave of Sea Peoples. The most widely followed theory, the middle chronology (MCC), would date Mycenaean IIIC Early to about 1175 B.C.E. and the Bichrome that developed from it to the mid-12th century B.C.E. This theory is becoming more problematic in light of the increasing consensus that Mycenaean IIIC:1b should be equated to Mycenaean IIIC Early to Middle, which should be dated to the mid-12th. The low chronology theorizes that Mycenaean IIIC:1b appeared only after about 1140 B.C.E. and the date of

the Bichrome pottery even later. Killebrew concludes that, with “some minor revisions,” the low chronology would best fit the dating of Mycenaean IIIC Early–Middle at other sites in the eastern Mediterranean and would also provide a more reasonable dating of Bichrome to the 11th century (continuing into the 10th). This conclusion is based on the LB II–Iron I stratigraphic sequences at both Tel Miqne and Ashdod.

This provides some support for the interpretation of I. Finkelstein (1995b) and D. Ussishkin, that the Philistine settlement was later than Rameses III’s 8th year. For example, most recently Ussishkin (2008) argues that, for all practical purposes, no Philistine pottery—Monochrome or Bichrome—was found at Lachish, even though such pottery was uncovered at the not-too-distant sites of Tel Zafit and Tel Miqne. This indicates that Philistine pottery post-dates the Lachish settlement, viz., 1130 B.C.E. This argument (albeit, one from silence) argues against the “Mazar–Singer–Stager hypothesis” that lack of “Philistine” pottery is due to cultural factors. A. Mazar (2008) responds that Ussishkin’s and Finkelstein’s arguments that contemporary sites should yield similar pottery assemblages in a geographical zone is correct in principle but should not be rigidly applied when dealing with specific cases such as this. They claim that it is inconceivable that local Mycenaean IIIC pottery did not reach contemporary sites in Philistia and the Shephelah, but this ignores cultural factors that could limit particular pottery to a few urban centres. They think lack of such pottery means a settlement gap, but such a widespread occupational gap is unfeasible and also negated by finds at such sites as Gezer. The early stage of Philistine settlement lasted perhaps only a generation. Lachish is at least 25 km from the major Philistine cities, sufficient to create a cultural border. Ussishkin, however, thinks that the data beg four questions: (1) How could the immigrating Philistines have come down the coastal highway as long as it was guarded by Megiddo? (2) How could the Egyptians have maintained control of Lachish, Megiddo, and Beth-shean if they did not control the coastal plain? (3) How could Lachish flourish so close to Philistine Gath without any pottery being imported between them? (4) How could Lachish conduct extensive trade with the coastal plain and the ports without any pottery being imported as well? The inevitable conclusion (according to Ussishkin) is that the Philistine settlement of the coastal plain must postdate 1130 B.C.E.

These differences between archaeologists may seem confusing, but they are no more significant than those between interpretations of the inscriptive data. What we have to recognize are some major divergences between the various models for understanding the settlement of

the Sea Peoples and the early history of the Philistines. The archaeology is vital, but it is also very interpretative at certain points. Yet the archaeology also provides data that seem to give a much clearer picture in certain areas. To take an example, Finkelstein (1996b) has compared the settlement patterns in the coastal plain between the LB and the Iron I, and this appears to lead to more definitive conclusions: in the LB the area of the coastal plain that became Philistia seems to have been the most densely populated in Palestine, with many sites of a variety of sizes (ca. 100, covering about 175 ha, though there were more sites in the Shephelah than in the coastal plain). About 80 percent of the Iron I sites had been settled in the LB, but there had been a drastic decline in numbers, down to about half the number in the LB. On the other hand, the proportion of large sites is up (from about half to three-quarters), while medium-sized sites almost disappear. Even though the number of sites is considerably reduced, it is mainly the smaller sites that no longer exist. Because there are proportionally larger sites (even though fewer), the total built-up area remains much the same. The result is a major reduction in rural sites but a considerable expansion in urban settlement.

Finkelstein goes on to note that the major LB sites were Gezer, Gath, Lachish, Ashkelon, Gaza, and Yurza. These continued into Iron I, with two exceptions: Lachish was abandoned (as was Tel Harasim), but Tel Miqne-Ekron had grown considerably. Ashdod and Ekron appear to be the strongest towns in the region, with Ashkelon and Gath the weakest (unless Ashkelon controlled Jaffa). The estimated population is 35,000 for LB and 30,000 for Iron I. This compares with 44,000 for the central hill country in Iron I (but only 2200 of these were in the Judaean Hills). Considering that few new sites were established in Iron I, and the total built-up area was also very similar, this suggests that the number of new settlers—the Philistines—were few. This confirms what a number of scholars had been arguing: the Philistines settled among the indigenous Canaanite population, perhaps as an elite. One result of this is that the material culture in the region included both Philistine and Canaanite elements (Bunimovitz 1990). But if the Philistines were so few, this argues against a major invasion force or even a large-scale migration. Whether some portion of the Sea Peoples destroyed the Hittite empire (cf. Singer 2000), Ugarit, and other kingdoms along the Mediterranean is perhaps a separate question, though not unrelated.

When we try to track the Philistines into Iron IIA, we run into difficulties because, as L. Herr points out, our archaeological information is poor (1997, 129, 131). We are thrown back on the biblical text with all the problems noted for it elsewhere. If we follow it, the Philistines

threatened Israel and Judah. It is probably that the Philistines controlled the Shephelah during this period, but it is unlikely that they were interested in moving into the hill country, since the “carrying capacity” of the coastal plain had not been reached (Finkelstein 1998b, 24). If there was a clash between Israel/Judah and the Philistines, the former might well have initiated it. But would the highlanders have had the strength or resources to do so? It seems unlikely (cf. Finkelstein 1998b, 24; 2003, 78). The occasional raid, yes, but not a wholesale invasion of the Philistine territory.

From Settlement to Statehood

The Question of the Settlement: Recent Models. Over the past thirty to forty years a great deal of debate has centred on the question of how Israel got into the land and eventually became a nation (see the survey in Ramsey 1981; McDermott 1998; Killebrew 2006). Until the 1960s most scholars favoured one of two models, the “Conquest Model” of the Albright School and the “Peaceful Infiltration Model” of the Alt–Noth School. Then, in 1962, a programmatic article by G. E. Mendenhall in a semi-popular journal outlined a third theory, but its real development came in the late 1970s: the Mendenhall–Gottwald theory of an internal revolt. Much debate has centred on these, and understanding them is still important for getting at the historical situation.

The Albright model of a unified conquest was heavily influenced by the biblical text, though it would be incorrect to conclude that it was only a paraphrase of Joshua. Albright initially dated the conquest to the 15th century B.C.E., but then N. Glueck conducted a survey of the Transjordanian region in the years 1938–40. Since Glueck concluded that these regions were uninhabited until the 13th century, Albright redated the conquest to the 13th century, a position he continued to maintain until his death. Although ostensibly based on archaeology, it had an uncanny resemblance to the first half of Joshua. In some ways, this was more apparent than real, but it allowed the conservative tendency to maintain academic respectability while still really following the biblical text. In 1977 J. M. Miller gave a thorough critique of the Albright position and ultimately rejected it (Hayes and Miller 1977, 254–62, 270–77; also Miller 1979). His conclusions presented a picture emphasizing the development of Israel from internal populations and also from the Sea Peoples or tribes forced to migrate in their wake, and expressed some scepticism toward the significance of nomadic incursions. The Albright position continued to be stoutly defended by Y. Yadin, even in one of the last of his articles to be published (Yadin 1979, 1982), but by then it was

being widely abandoned by scholars. There have been a few die-hard defences by conservative-evangelicals (e.g. Wood 1990).³ Thus, one is somewhat disconcerted to find a mainstream archaeologist trot out the thesis again, though he argues that he is only listing this as one of several possibilities (Ben-Tor 1998; Ben-Tor and Zuckerman 2008, 2; contrast Ben-Ami 2001). The “Conquest Model” is now only of historical interest, but it should alert scholars to the fact that vociferous adherence by large numbers of academics is no guarantee that a particular theory will stand the test of time.

The “Peaceful Infiltration Model” of Alt and Noth has better stood the test of time in certain ways than Albright’s. It continued to find adherents among archaeologists in the 1980s and even the 1990s (e.g. Zertal 1994). Yet a number of criticisms had been levelled at it that rendered the thesis in its original form untenable. One was that the desert could not produce sufficient nomads to populate the hill country in the Iron I (see Chaney 1983; but this is disputed by Finkelstein 1988, 308). Another was that nomads adopt their lifestyle out of necessity and want nothing more than the chance to settle down: in fact, the “nomadic” mode of life takes many forms, and those who engage in it are no more likely to change than those in other forms of subsistence agrarian activity (see the discussion in Lemche 1985, 136–47).

The “Mendenhall–Gottwald thesis” is widely used to designate the internal revolt model. Gottwald’s 1979 study was the academic underpinning for Mendenhall’s programmatic article almost two decades earlier. A collection strongly espousing the Mendenhall–Gottwald thesis was notable for the trenchant essay by Mendenhall attacking and disowning Gottwald’s approach (Mendenhall 1983, 91–103). Although noting some of the genuine differences between the two scholars, it has not stopped the hyphenated designation for this model. N. P. Lemche, while expressing his appreciation of Gottwald’s use of sociology, presented a major critique of his work (1985; also Finkelstein 1988, 306–14). Although differing from Gottwald on many points, including the concept of an internal revolt, Lemche’s final picture was of an internal development of

3. Brief mention should be made of John Bimson (1981) who also attempted to resist the rapidly disappearing invasion under Joshua. The views of Albright and others became dominant in their interpretation that Joshua came at the end of the Late Bronze. Bimson’s main contribution to the debate was to show how fragile the archaeological support was for the Albright thesis, although his aim was to undermine Albright’s dating rather than the conquest model as such. He wanted to date Joshua to the end of the Middle Bronze, as Albright originally believed, but his thesis has not gained any real following.

the indigenous population. In his view, though, it was not urban residents who fled oppression of the city-states but members of the rural peasantry who went up into the hill country to escape taxes (1985, 427–32).

The three models that dominated the 20th century are no longer equal alternatives. An examination of their strengths and weaknesses has already suggested which way scholarship has moved. The Albright thesis of a unified conquest has all but been abandoned by mainstream critical scholarship and continues to be pressed only by a few conservative scholars. The reason is that much of the support has simply evaporated, in particular the evidence that was once seen as its main strength: the archaeology. In the past quarter of a century most of the discussion has revolved around the Alt–Noth and the Mendenhall–Gottwald theories and has focused on the archaeology and on social-scientific models. Neither model is seen to be adequate in itself, and much of recent thinking has combined aspects of both.

A number of variants of these three have subsequently been developed. The “symbiosis theory” of V. Fritz (1981, 1987) is really a variant of the Alt–Noth thesis, though it also incorporates insights from Mendenhall–Gottwald. Fritz points to the similarity of the material culture of the highland settlers to that found in the territories of the Canaanite city-states, but also differences. This shows that the new population (assumed to be pastoralists) was not the same as the Canaanites but was in close contact with them over several generations. This population eventually settled down, their material culture showing a lot in common with Canaanite culture but also with enough differences to distinguish them.

I. Finkelstein produced what seems to be a true merging of Alt–Noth and Mendenhall–Gottwald, with a firm archaeological base (Finkelstein 1988). Much of his study is on the archaeology of developments during the Iron I and documents the unprecedented growth of population in the hill country through this period. Although accepting that the new population included a number of elements, he especially argued that the spectacular growth came about because a large nomadic population settled down. The nomads did not come from the “deep” desert region, however, but were the descendants of those who left settled life for a pastoral lifestyle in the Middle Bronze because of adverse conditions. He argues that the region went through a regular long-term cycle of people moving from settled life to nomadic life and back again, depending on climatic and economic cycles. Thus, the new population of the hill country was made up of nomadic pastoralists different from the Canaanites (as Alt–Noth proposed), but they were part of the indigenous population (as argued by Mendenhall–Gottwald). Finkelstein notes that the

earliest settlements in the areas of Ephraim, Manasseh, and Benjamin were on the desert fringe; they also often settled near Canaanite cities (1988, 310).

Finkelstein's thesis has been critiqued mainly on the basis that the archaeology of the Iron I hill country settlements does not point to nomadic influence on the material culture (Dever 1998a, 1992). Also, the studies of pastoralists indicate that pastoralism is often not an exclusive lifestyle but one activity of people who also engage in agriculture (Lemche 1985, 136–47; Killebrew 2006, 565). This is true, but it does not strike at the heart of Finkelstein's proposal. More important are studies of Palestinian nomads in recent times who lived and moved freely among the settled areas and peasant farmers. Dever refers to these in his criticisms of Finkelstein (Dever 1998a, 222–29). As G. Lehmann (2003, 155) points out, however, "It is impossible to estimate the number or the impact of nomadic pastoralists in Iron Age Judah." More important is the context argued for by Finkelstein: the alternating cycles of sedentary agriculture and pastoralism that he sees as determining settlement and the lifestyle of the inhabitants in Palestine from at least the 3rd millennium B.C.E.

W. G. Dever discusses "yet another attempt at synthesis" in which he develops the model of "a frontier agrarian reform movement" (2003, 167–89). A variety of dissident groups withdrew to the frontier area, simply in quest of a new society and new lifestyle—a chance to start over. The groups included urban dropouts, *'apiru* and other social bandits, refugees of many kinds (including displaced villagers and impoverished farmers), and local pastoral nomads. Yet the peasant revolt model seems particularly important for him (2003, 185–88). He also argues that land reform was both a driving force and the main goal of the "proto-Israelites." The importance of the agrarian element is no doubt correct, though the assumed urban/rural opposition is problematic (see Grabbe 2001). Unfortunately, no detailed integrated theory is set out, and it is not clear how the various elements cohere. For example, it is not obvious why peasants seeking land reform would set up farms in the frontier area. One would think that revolting peasants would be seeking to take over and redistribute the farm land around the Canaanite city states, not fleeing to the hill country. Withdrawal does not constitute a revolt. Also, Dever does not refer to Lemche's proposals about the peasantry (discussed above) or indicate whether he agrees or not.

N. Na'aman (1994) associates the origins of Israel with the wider developments in the eastern Mediterranean. The 13th to 11th centuries brought the settlement of peripheral areas contemporary with the collapse

of urban culture in the entire Aegean–Anatolian–Syro-Palestinian region and the migration of large groups on the boundaries of Mesopotamia. At no other time was the disruption of urban culture in Anatolia and Syro-Palestine (reaching as far as the Aegean and the Balkans) so complete as in the 12th century. The Arameans gradually took over large tracts in Mesopotamia and Syria. Large-scale migration in Iron I because of destruction in Asia Minor means that various groups reached Canaan and played an important part in settlement: Hittites in Hebron; Hivites in western Benjamin and perhaps around Shechem; Jebusites from the Hittite empire; Girgashites from Anatolia. Only two of the alleged seven pre-Israelite nations were autochthonous. These groups may have helped break Egyptian rule. Traditions about patriarchal migrations to Egypt are best understood against this background; similarly, stories of coming out of Egypt such as the exodus should be understood as vaguely remembered background rather than routine migration of pastoral groups. Thus, the overall picture does not support the assumption that the Iron I settlement was only an internal Palestinian one or one involving settlers only from local Canaanite elements. The various groups entering Canaan joined the local uprooted and pastoral groups, which led to an increase in pastoral and bandit splinter groups that upset the urban-nomad balance and induced the nomads to settle down: "The model that emerges from my analysis is of small and larger groups of variegated ethnic and cultural background who settled during a long period and slowly and gradually started cooperating in the new environment" (1994, 246).

Z. Herzog (2002, 89–92) draws attention to the current "fluid" concept of ethnicity that undermines attempts to identify ethnic borders. In the Beersheba Valley in the Iron I a variety of different groups seem to have lived (according to the biblical text), with a relatively uniform material culture. Here the groups mixed and combined in a complex social composition. Using the Beersheba Valley as a model, Herzog argues that the larger settlement and the emergence of an Israelite identity should be understood from an "interactive-combined model" in which "a community identity is created from the development and combination of various social groups. Clearly, in different regions there were different combinations of communities, simultaneous with inter-regional mixing and blending" (2002, 92).

A. E. Killebrew (2005, 149–96; 2006) has argued for the origins of Israel as a "mixed multitude." Although she gives a succinct and persuasive argument for her case, she admits that the thesis is not original with her. Indeed, it has been adumbrated in some form or other (though often briefly and without supporting argumentation) by a number of researchers

going back at least to the mid-1980s (Ahlström 1986, 57–83; Dever 1992; 2003, 181–82; Finkelstein and Na’aman, eds., 1994, 13–14). According to this theory, the population that became Israel was made up of a diverse group of people:

...demographic redistribution and an increase in the settled population, especially in the central hill country and Transjordanian highlands. These inhabitants most likely comprised different elements of Late Bronze Age society, namely, the rural Canaanite population, displaced peasants and pastoralists, and lawless *‘apiru* and *shasu*. Outside elements probably included other marginal groups, such as fugitive or “runaway” Semitic slaves from Twentieth-Dynasty New Kingdom Egypt... Other nonindigenous groups, such as Midianites, Kenites, and Amalekites, perhaps connected with the control of camel caravan trade routes between Arabia and Canaan, may have constituted an essential element of this “mixed multitude.” (Killebrew 2006, 571)

The recent study of E. van der Steen (2004, 306–10) lends support to this thesis. She argues that the trading collapse in Ammon caused inhabitants to migrate not only into Moab but also into the Jordan Valley. There the pressure of population caused further migration into the hill country west of the Jordan. It may be that there was a partial vacuum there at this time because Merneptah (or Rameses II) had “laid waste Israel” (Merneptah Stela) who may have been living in the northern hill country.

Israelite Statehood and Anthropological Models. The question of statehood for Israel and Judah cannot easily be disassociated from that of the settlement: the settlement and the rise of the monarchy seem ultimately to be part of same process. This does not suggest that, once those who became Israel settled down, a state was inevitable, but the settlement—its nature, its development, the resources available—are all determining factors in the progress toward statehood. One followed from and presupposed the other.

In recent years there has been much discussion among anthropologists and archaeologists about the nature of states, the types of states, how states develop and what precedes them. It is not unusual for discussions about the rise of Israel to make some reference to this discussion, but it must be said that most writings by biblical scholars do not give a thorough treatment of the subject. Usually, anthropological studies are mentioned for the sake of selecting one as a model and following it without critiquing others or even indicating why others are not appropriate. It is fair to say that when anthropological studies on state development are

mentioned, it is not infrequent to expound one and follow it without serious discussion of other possibilities or critique of the one being followed. The literature on the subject is quite substantial, especially if one takes into account everything written since the seminal work by M. Fortis and E. Evans-Pritchard (1940). No attempt will be made here to trace the discussion over the decades, but several points can be made in order to summarize the most important consideration for discussion in the present:

1. Much of the discussion, at least until recently, has been based on evolutionary models (Redman 1999, 49–53; Gosden 1999, 476–82; Renfrew and Bahn 2004, 178–82; Yoffee 2005, 22–41). A popular one have been the model of E. R. Service (1962) in which civilization developed through the stages of bands, tribes, chiefdoms, and finally states. Evolutionary models do not have to force everything into a rigid pattern nor suggest inevitability (Feinman and Marcus, eds., 1998, 5–6). N. Yoffee fully accepts the concept of social evolution, but his recent study is a sustained critique of the widely regarded “neo-evolutionary model” (Yoffee 2005, 22–41).

2. The various models of statehood are ideal types, but there is a danger of elevating ideal types to reality. Reality is always much more complicated, while ideal types only serve as a means of investigation and comparison. Within the various types there is a great deal of variation. To pick one sociological model and argue from it without considering others or without critiquing the models themselves is to be naive and uncritical.

3. The concept of chiefdom—so beloved of studies on the development of statehood in Israel and Judah—has been much criticized (Yoffee 2005, 22–31). See further on this below.

The types of early states have been discussed in the anthropological literature, with a bewildering variety of models: city-states, territorial states, regional states, segmentary states, tribal states, ethnic states. The term “city-state” frequently comes up in discussions relating to ancient Palestine, yet a recent collection found the term problematic (Feinman and Marcus, eds., 1998, 8–10). Conversely, B. Trigger (2003, 92–113) divides early states into the “city-state” and “the territorial state”; yet, disconcertingly, toward the end of his discussion he suddenly introduces the “regional state/kingdom” without discussing what it is or how it relates to the other two. “Regional state” is also used by T. J. Barfield (1990), but he seems to be using the term in a different sense from Trigger.

A number of the recent theorists on the rise of the Israelite and Judahite states have worked with the model of the chiefdom. Of recent works R. D. Miller II argues that Israel began as a “complex chiefdom” (2005, especially 6–14):

those having an intermediate level or levels of “subchiefs” between the paramount and the people... It is complex chiefdoms that develop into states and that one would expect to find in Israel on the threshold of Monarchy. (Miller 2005, 8)

Three main characteristics are said to be associated with complex chiefdoms: (1) tribute mobilization, (2) cycling (between two or three levels of control), and (3) sacralization. Unfortunately, Miller does not appear to take account of the debate. For example, he makes little reference to the critique of the chiefdom concept (e.g. Yoffee 2005, 22–31) and none to those who argue that the chiefdom is an alternative to the state, that is, that chiefdoms do not develop into states (cf. Yoffee 2005, 26–27). His statement that it is the complex chiefdom that develops into the state also does not seem to recognize that the complex chiefdom might well cycle into a “simple chiefdom.”

N. Yoffee (2005) both critiques the “neo-evolutionary” thesis and also makes his own proposals about the development of the state. His thesis seems to be expressed in the following statement (2005, 32–34):

new social roles and new forms of social relations emerged alongside, and to an extent supplanted, exclusive kinship rules (of marriages and the status of children) that also functioned as the framework for relations of production. Leadership, exercised by shamans, expert hunters, and charismatic individuals, gave way to formalized ideologies in which the accumulation of wealth and high status were seen as rightfully belonging to leaders whose roles were, among other things, to “make inequality enchant.” As social relations were transformed into relations of domination, new ideologies led to the acquiescence of subjects in their own domination and the production of their own subordination... The new ideologies of state, which were inextricable from the changing social relations that gave them birth, thus depicted how dominant leaders “served” those who daily and perpetually served them... The earliest states, thus, consisted of a political center with its own leadership structure, specialized activities, and personnel, but also included numerous differentiated groups. These social groups continuously changed in their organization and membership in relation to the needs and goals, strengths and weaknesses of the political center.

According to Yoffee (2005, 34–38) the critical processes of social and economic differentiation and political integration come about through the various forms of power that had to be in place before the earliest states could evolve: economic power (agricultural production, mercantile

activity), social power (elites at both the state and local level, creation or adaptation of symbols of cultural commonality), and political power (bureaucratic administration, military organization, legal system, taxation structure). Although ancient states were unlikely to have passed through a chiefdom-like stage, he accepts and even presses the concept of city-state (2005, 42–62). He mentions in passing “Yoffee’s rule”: “If you can argue whether a society is a state or isn’t, then it isn’t” (2005, 41).

A. H. Joffe (2002) proposes the model of the “ethnic state” for the rise of secondary states in the southern Levant, meaning Israel, Judah, Ammon, Moab, and finally Edom. By “ethnic state” he means

polities integrated by means of identity, especially ethnicity, and which are territorially based...they are novel and historically contingent political systems which appear in the Levant during the first millennium B.C.E. thanks to the confluence of several factors, not least of all the collapse of imperial domination and the longstanding city-state system. (2002, 426)

Joffe focuses on the archaeology and extra-biblical texts rather than the biblical accounts.

Based on fragmented evidence from the material culture (e.g. a series of palatial structures in *bit-hilani* style; proto-Aeolic capitals; red-burnished Phoenician-style tableware) Joffe identifies a state in the Cis-Jordan region already in the 10th century (2002, 440–46). The similarity of construction styles at Megiddo, Hazor, and Gezer indicates they were part of a larger political unit, but all three seem to be in border areas. What the centre might be is not known. In spite of some marks of royal ideology, other significant ones are missing: representational art, monumental inscriptions, inscribed or decorated objects, inscribed or uninscribed seals, or weights. Although Joffe insists on calling it a state, he admits it was fragile, hardly integrated, and had little in the way of meaningful ethnic unity. In any case, much depends on following the conventional dating: if the LC turns out to be correct, the postulated development of the ethnic states will be somewhat different.

H. M. Niemann has produced another model, in which he argues that the kingdom of Omri was the first state in Palestine (1993, 2007). David was only a tribal chief, while Solomon functioned much as the lord of a Canaanite city. It was Jeroboam I who consolidated the tradition of mountain rulers in the Northern Kingdom, but Israel never managed to fuse the various tribal and cultural elements into a unity. Niemann interprets Omri and his successors as mobile warrior leaders, which is reflected in the structure of the Northern Kingdom. Less emphasis was placed on a permanent centre (Samaria was not a capital city, for example), but society remained largely tribal (as it had traditionally been)

and precedent was put on maintaining the military strength. Samaria served as an administrative unit, but administrative structures were not well developed (e.g. no network of civil servants across the country). There were a few specialized centres with differentiated functions (trade, cult, defence), but these formed only a loose network of (mainly military) sites, primarily on the borders. Samaria was the only royal foundation in the heartland (though the traditional cultic sites of Dan and Bethel received royal support).

Other types of early state are also sometimes postulated. The term “segmentary state” seems to have been coined by A. W. Southall (1956). In a more recent article, J. W. Rogerson argued that ancient Israel was not a segmentary society but approached being a segmentary state (1986). The concept has come under considerable criticism, and Southall himself eventually gave it up (Marcus and Feinman 1998, 7–8). Another is “tribal state,” of which three sorts are proposed: (1) one tribal elite or dynasty conquers and rules over a heterogeneous population; (2) a non-tribal dynasty is brought to power by and depends on tribal support; (3) the rulers attempt to eliminate tribalism but promote a national ideology of integration that resembles tribal ideology (Tapper 1990, 69). Inspired by M. Weber, L. E. Stager (1985, 25–28), followed by D. Schloen (2001) and D. M. Master (2001), proposed the archetype of the patrimonial state. This is modelled on the household, in which leadership is vested by tradition in the patriarchal figure. In a recent book on the rise of Moab as a state, however, B. Routledge considered but rejected both these popular “local models” of state formation, “tribal states” and “patrimonial states” (2004, 115–23).

One of the points made in a number of studies is the importance of ideology to the founding of states (Caton 1990). “Ideology” can of course cover a number of different perspectives, including that of religion. For example, E. Gellner (1981) has demonstrated the significance of saints and religious leaders for the development or maintenance of chiefdoms in early Islamic history (also Tapper 1990, 65). In this light, the suggestion of Gottwald and others that Yahwism was important to the development of early Israel takes on a new significance. We also have to consider the place of law, which can play a similar role. In Israel law included not just civil law but moral and religious law. Unfortunately, it becomes a matter of speculation at this point because we have so little evidence.

The Saul, David, and Solomon Traditions. It has often been assumed in the past that with the reigns of Saul and David we begin to get into the historical period. In Noth’s classic theory, 1 Sam 13 to 2 Kgs 2 was

thought to have been made up of several blocks of tradition that were given only minor editing (including some formulaic introductions and transitional passages) before being incorporated into the DtrH (Noth 1981, 54–57). More recent analysis has not been as optimistic about finding early narratives, but the stories of Saul and David show evidence of a variety of traditions: there is pro-Saul bias in the Saul traditions and anti-David perspectives in the Davidic material. There are also some essential differences between them and the Solomon narrative.

To begin, let us note some salient points about the Saul tradition. First of all, Saul looks like a chieftain, with a court that meets under a tree (1 Sam 22:6). We have two versions of how he became king: one is that he was anointed by Samuel (1 Sam 9:1–10:16 [23] + 13:2–14:52), which looks like a biased account from a prophetic source that wants to make Saul subordinate to Samuel; the other—more likely to be reliable—is that he arose as a deliverer (1 Sam 11:1–15). Whether there was an actual Philistine threat in his time is debatable, but it is possible. The extent of Saul’s territory was probably the same as that of Eshbaal (2 Sam 2:9) and included the central highlands. Many of his activities could have been accomplished in two years (cf. 1 Sam 13:1).

A recent analysis of the Saul tradition finds a historical core, though this has been filtered through the distorting lenses of Davidic court circles, prophetic circles, Deuteronomistic perspectives, and anti-monarchic views (Shalom Brooks 2005). The stories of killing the priests of Nob or the Gibeonites are probably later calumnies. According to Shalom Brooks, the population in the central highlands was already moving toward a new socio-economic situation characterized by a developing centralization. This was the background for the rise of the monarchy. Saul was a successful leader, the first to develop a standing army, who had the support of the people, including those of the Judahite hill country. Saul was not only able to unite the Israelite tribes but also to incorporate Canaanites and other minority groups into the emerging state. Loyalty to Saul continued after his death, creating rebellions and other problems for David; indeed, David almost wrecked the monarchy by his sabotage of Saul’s rule in order to gain the throne for himself.

Similarly, D. Edelman (1991, 1996) sees the historical Saul as the petty king of Gibeon with Benjaminite roots who expanded into surrounding territory to create a state called “Israel.” Attempts to control local trade routes and find markets brought him in conflict with the Philistines and other independent states. He died trying to expand into the Jezreel–Beth-shean corridor. She (1996) suggests a number of sources (several of them oral) embedded in the DtrH account: (1) *Mashal* (1 Sam 10:11–12; 19:24); (2) song fragment (1 Sam 18:7; 21:12); (3) lament

over Saul and Jonathan (2 Sam 1:19–27; most of the information in 1 Sam 28–31 is taken from lament); (4) fragment of an administrative list (2 Sam 2:9); (5) Saulide genealogy (1 Sam 9:1; the basis of 14:46); (6) an old tradition underlying 1 Sam 9:1–10:16; (7) traditions linking Saul with Gibeon (Eshbaal younger than 40); (8) use of a northern source to create a Saulide narrative (Gibeah vs. Geba).

A number of recent studies attempt to analyze the David story (Brettler 1995, 91–111; McKenzie 2000; Halpern 2001; Finkelstein and Silberman 2006). The David traditions are in part bound up with the Saul traditions, and they need to be evaluated together. One of the concerns in the early part of the story is to legitimate David—strongly suggesting that David was a usurper (Shalom Brooks 2005, Chapter 4). To take one analysis, N. Na’aman (1996) argues for some written material (lists, extracts from a chronicle of kings), though not all necessarily from David’s time. It was not customary at this time to cite documents. These probably came from the scribal milieu where such documents were used for training. The verdict is still out, however, as to whether he ruled over a large kingdom.

In the biblical story, David fits the image of the hero figure; there are many folkloristic elements and a variety of traditions; yet there are also traditions with some interesting twists, such as the willingness to acknowledge some of David’s weaknesses, the need to legitimate David from a variety of angles—suggesting that he was not seen as legitimate by everyone—and the admission that David did not do certain things that we might have expected. To summarize, we can note some of the points that emerge from a look at the Saul and David traditions:

- The tradition recognizes that David was not the first king.
- Saul came to the throne probably as a military leader by popular acclaim (1 Sam 11:1–15), whereas the prophetic tradition that the king was subject to Samuel’s choice and censure (1 Sam 9:1–10:16, 23; 13:2–14:52) is unrealistic.
- The apparent boundaries of Saul’s kingdom (2 Sam 2:9) are reasonably in line with the natural and demographic resources in Cis-Jordan.
- A strong link is made between David’s rise and Saul’s court, but much of this looks like a deliberate attempt to legitimate David as king from a variety of angles: anointing by Samuel (1 Sam 16:1–13); armour-bearer in Saul’s court who plays the lyre for him personally (1 Sam 16:14–23); slaying of Goliath (1 Sam 17); marriage to Saul’s daughter (1 Sam 18:17–27).

- Although David is mainly presented as a positive—even ideal—figure, several significant “sins” or weaknesses maintain a place in the tradition, as well as rebellions against his rule. Also, some alternate traditions are preserved, such as another slayer of Goliath (2 Sam 21:19).
- Contrary to expectations David does not build a temple (though a strenuous effort is made for him to do everything short of the actual building).
- Both Saul and David were mainly military leaders.
- The text itself does not suggest an extensive administrative apparatus in the case of either Saul or David.

The narrative about Solomon’s reign (1 Kgs 2–11) seems to be rather different from those about Saul and David. One is immediately struck by how uniform it is. Almost from start to finish Solomon fits the image of the great “Oriental emperor.” He controls a vast territory and possesses great wealth, with absolute sovereignty over his subjects. Of course, he marries the daughter of a country of similar power—suggesting equality with Egypt in this case—and harnesses the best craftsmen and materials from legendary Tyre to build his city. His capital city consists of great palaces and a magnificent temple, with gold like dust and silver so abundant it is of little account. His household overflows with luxuries, his table groans under the weight of exotic fruits, meats from rare animals, and every sort of desirable food for consumption. His wisdom is legendary, and he exceeds all others in intellectual skills. His reputation reaches far and wide, and rulers from distant lands travel to see such a supreme example of power, wealth, and wisdom—only to find that the reports were understated. His ships travel to the ends of the earth for rare and astonishing goods.

E. A. Knauf (1997) thinks that Solomon was historical but that he differed considerably from the biblical picture. The king’s name shows that he was non-Judahite in origin. The Bathsheba story was not suppressed because there was a worse story: Solomon was not David’s son. Knauf has tried to reconstruct some early sources. He sees 1 Kgs 8:12–13 as an early text quoting an incomplete royal building inscription which confirms Solomon’s place in association with the temple (though perhaps only establishing a cult in a pre-existent temple rather a temple builder as such). In 1 Kgs 1–2 (a text probably from the 7th century) he sees glimpses of Solomon as the puppet of the Jerusalem elite on whom he turns and eliminates. He concludes that Solomon was the son of a Jerusalem mother but not necessarily of a Judahite father. He became

king through a *coup d'état* by getting rid of the Jerusalem elite. He was no monotheist, because the Judaean tribal deity Yhwh had only a subordinate position in the Jerusalem pantheon.

N. Na'aman (1997) also analysed the account of Solomon for the existence of sources. He accepts that the early "chronicle of Israelite kings" was created in the 8th century and thus rather later than Solomon; however, the Deuteronomist also had the "Book of the Acts of Solomon" (1 Kgs 11:41), which Na'aman tries to reconstruct. Some episodes were invented by the post-Dtr redactor, including the Queen of Sheba and the description of the temple (based on a description of the temple of his own times). Na'aman concludes that only in the late redaction do we have a picture of a ruler of an empire and a great sage. Although I agree that there was a "Chronicle of the Kings of Judah" (Grabbe 2006a), I am more sceptical of a "Book of the Acts of Solomon." If such existed, it might have provided some historical facts, but we could not be sure without a careful analysis. But were such writings being produced this early? Administrative documents, yes, but not the sort of biographical writing envisaged here.

I find it difficult to discover much in the Solomon story that strikes me as likely to be historical. I do not discount the existence of a King Solomon. His name echoing the old god of Jerusalem (Shalim/Shalem) is suggestive of reality rather than simply the piety of the David story. Also, he began his reign with the bloody elimination of rivals, though the idea that he took his throne in the midst of adversity which he overcame could be a part of the stereotype, and the writer probably saw nothing bad in this. Overall, I can find little in the Solomon story that looks on the face of it to be historically reliable. Yet I am intrigued by the story that he built the Jerusalem temple. This sort of story is what we might expect, and the description of the wealth and rare construction of the temple fits well the legend. Yet David—the expected temple-builder—did not construct it, and we find nothing in the stories of the later kings that might hide such a building (with the possible exception of Jehoash who is said to collect money to repair the temple: 2 Kgs 12). This suggests that a temple was built in Jerusalem at a fairly early time. If David did not build it, who? Possibly here we have a genuine remembrance that has been expanded into a great legend.

Thus, here and there might we might find a verse that reflects the historical Solomon, but to my mind the Solomon story is the most problematic, providing the thickest cloud of obscurity over the history that lies behind it.

III. Conclusions

Here are some summary observations about the growth and development of the land of Palestine between the LB and the Iron II, including the rise of the states of Israel and Judah:

1. The growth in the hill country in Iron I no doubt lay at the roots of what later became Israel. Yet it should be noted that this growth was not in the south but the north.

A critical mass of settlers was reached without which certain things could not happen, but this was more significant in the north and decreased as one went south, with the area south of Jerusalem still being mainly pastoral. Settlement in the Judaean hill country was sparse in the Iron I. Growth in the area of Jerusalem and to the south came only later.

2. The location of Merneptah's "Israel" is not known, except that it seems to have been in Palestine. Something called Israel existed in the Palestinian region about 1200 B.C.E., and it appears to have been a people. Exactly where this people lived, what/who constituted it, where it got its name, and its relationship to the Israel of the Bible are all questions. One place we can say that it was *not*, though, was in the Judaean hill country. This seems evident in that Judah is never referred to as "Israel" by outsiders until long after the fall of Jerusalem in 70 CE. The area known as "Israel" was always the northern area, the area that became the Northern Kingdom.

3. The growth in the hill country seems to be owed to a variety of sources, including pastoralists from both Trans-Jordan and Cis-Jordan, various ethnic groups, some refugees from Canaanite cities, but mostly to indigenous peoples—"Canaanites"—whose background seems to have been mainly agrarian. It seems that at some point a dominant ethnic consciousness came about in this region. Part of the reason might be by force: an "Israelite" group might have conquered or otherwise taken over some other smaller groups and assimilated them (as suggested in Joshua and Judges). It has long been suggested that a number of innovations in technology took place in the central highlands: terracing, plastered cisterns, the "Israelite house," collared-rim jars, and the use of iron; also the absence of pig husbandry has been seen as significant. Unfortunately, it seems there is no list of technologies or practices that is exclusive to this region and time or that can serve as "Israelite ethnic markers." On the contrary, it seems that a variety of ethnic groups (Hittites, Hurrians, Jebusites, Grgashites, Amorites, Shasu [?], et al.), as well as social elements (*apiro*, Shasu [?], pastoralists settling down, peasants fleeing the lowlands, et al.), settled the hill country on both sides of the Jordan in the Iron I, if our written sources are anything to go by.

4. Was this sufficient to cause these disparate groups to coalesce into the early “Israelite”? It is difficult to say, but we must also take account of another possible factor: religion. One of the characteristics—and constituents—of an ethnic group is a set of common myths and ideologies, which can include religion. Here the suggestion of Mendenhall, Gottwald, and others might have some theoretical validity, even if they provided no concrete evidence, that the catalyst was Yahwism. One is tempted to dismiss this proposal because of the way in which previous generations have privileged biblical religion—which we now generally agree was not necessarily the same as Israelite religion. Yet other arguments (from the social sciences) may give support to the idea.

Israelite society was long polytheistic, but Yhwh did function in some way as a national god and seems to have been the most widely honoured deity. Thus, it is possible that Israel partly coalesced around Yhwh but, even if true, this would have been only one factor; it would be simplistic to suggest that this was the only factor. Indeed, we do not know for certain to what extent the hill peoples saw themselves as a single ethnic group, but the question becomes relevant if we start discussing Israel as an “ethnic state” (as discussed above).

5. Exactly what led to the development of a state in Palestine can be debated, but the old argument that it was the Philistines, perhaps combined with increasing population, is to be strongly questioned in the light of recent anthropological debate on the issue of state formation. The view that a chiefdom must have preceded the state is also to be questioned. Although large cities often preceded the state in examples examined by Yoffee (2005), this is not necessarily the case. Here, it is difficult to find large cities that led to an Israelite state. None of the cities of Palestine were large in the way described by Yoffee. If Yoffee is correct, we should be looking for economic, social, and political sources of power (2005, 34–38). For many anthropologists, the formation of the Israelite and Judahite states would probably be explained from impersonal social and political forces, but others would allow a personal element into the mix, such as the presence of a dynamic charismatic leader.

For example, Coote and Whitelam (1987) explain the rise of Israel as a state from the combination of pressure on highland resources as population expanded and the pressure from the Philistines in the lowlands. This follows a long line of scholars seeing the “Philistine threat” as in some way pressing the Israelites into initiating a monarchy. Yet there are some objections to these proposals. First, there is no evidence that the “bearing capacity” of the highlands had been reached; in other words, archaeological surveys suggest that the highland population could still

have been sustained by local resources without having to expand: the same population or greater could be found there in other periods (cf. Finkelstein 1999a; Broshi and Finkelstein 1992). Secondly, was there any impetus for the lowlanders to expand into the highlands since they also still had room to grow (Finkelstein 1998b, 24)? Thus, if there was friction between the Israelites and Philistines, it is likely to have been the Israelites who initiated it—perhaps making raids on the Philistines. But considering this explanation inadequate does not answer the question of why the highland settlements coalesced into a state.

6. Precisely when a state developed in Palestine is difficult to say because of the current debate about chronology. Some of the indications of an “ethnic state” envisaged by Joffe in the 10th century are dated to the 9th by the LC. Thus, the early state that he sees might be the Omride kingdom—the political entity that is almost universally regarded as a state. Whether a state preceded Omride rule, though, is currently furiously debated. By rights we would expect the first secondary state in Palestine to arise in the coastal plain, but here the Philistine city-states were successful, and there seems to have been no impetus to form a larger unit. The next area would be the northern region, perhaps the northern valleys or hill country. It is precisely in this area that the Omride kingdom emerged, which is one of the reasons that some scholars think this was the first Israelite state. The problem is what to do with the biblical traditions about the rise of the Israelite kingship. The idea that Israel was under divine rule but had Samuel anoint a king chosen by Yhwh is of course pure theology, not history. Should we simply dismiss the whole of the biblical account as too difficult to deal with? This is perhaps tempting, and some have certainly followed this approach. Yet more considered analyses suggest that there is history behind the stories of the “united monarchy,” even if precisely what sort of history is a matter of debate.

7. It must be admitted that a good deal of the debate about individual details are in aid primarily of one goal: to determine whether or not there was a “united monarchy.” For some, the United Monarchy is a touchstone of some sort or other: some defend it because they wish to defend the Bible, and some oppose it because they want to discredit the Bible. Others have other reasons for defending or opposing it. For me it is purely a historical problem—I don’t really care one way or the other whether there was a United Monarchy. What I do care about is integrity of presenting the data and arguing the question, and I think there is a complex of arguments that have to be worked through. I believe there is sufficient evidence to assert that Saul, David, and Solomon existed.

As for the different views about the “United Monarchy,” some favour the position that there was such an entity and others oppose it. But when all is said and done, I fail to find a major distinction between the views espoused by Israel Finkelstein and Amihai Mazar. Mazar (2007, 164–65) states:

It is certain that much of the biblical narrative concerning David and Solomon is mere fiction and embellishment written by later authors... I would compare the potential achievements of David to those of an earlier hill country leader, namely Labayu, the *habiru* leader from Shechem... David can be envisioned as a ruler similar to Labayu, except that he operated in a time free of intervention by the Egyptians or any other foreign power, and when the Canaanite cities were in decline. In such an environment, a talented and charismatic leader, politically astute, and in control of a small yet effective military power, may have taken hold of large part of a small country like the Land of Israel and controlled diverse population groups under his regime from his stronghold in Jerusalem, which can be identified archaeologically. Such a regime does not necessitate a particularly large and populated capital city. David's Jerusalem can be compared to a medieval Burg, surrounded by a medium-sized town, and yet it could well be the centre of a meaningful polity. The only power that stood in David's way consisted of the Philistine cities, which, as archaeology tells us, were large and fortified urban centres during this time. Indeed the biblical historiographer excludes them from David's conquered territories.

Finkelstein (2001, 107–8) states:

...we may still be able to identify in them [the stories of David] the action of a local chieftain who moves with his gang to the south of Hebron, in the Judean Desert and in the Shephelah, far from the control of the central government in the highlands further to the north. David takes over Hebron, the second most important Iron Age town in the highlands of Judah and the centre of his theatre of operations, and then expands to the north and conquers Jerusalem, the traditional centre of government in the southern hill country. David, according to these stories, is a typical Apiru leader, who manages to establish a new dynasty in Jerusalem.

8. If we return to the processes that lead to state formation, we find that most of these would point to state formation in the northern part of the country. The natural resources of the country are more concentrated here, with greater scope for agricultural production. This in turn gave greater support for the military. The proximity to Phoenicia and the coast would have given encouragement to trade. Saul was evidently the first

king, was primarily a military leader, and was overthrown by David who subsequently met a lot of opposition, including from Judah. If anything resembling a united Israel—a territorial state—came about under David, it would have been an unusual development. One can hardly claim that it would be impossible, but it seems reasonable to conclude that in the light of all circumstances it would not be very likely.

On the other hand, perhaps a city-state, much like the city-states of Shechem under Lab'aya or of Jerusalem under 'Abdi-Heba, would be feasible. Much in the David tradition would be compatible with that. The archaeology does not yet support this, but most do not see the existence of a citadel of the ruler in an otherwise small settlement at Jerusalem as being contradicted by the archaeology currently available. But a Jerusalem city-state is not the same as the “United Monarchy” of the Bible. It seems unlikely that David controlled anything beyond a limited territory centred on the southern hill country and Jerusalem. This might have overlapped with territory earlier controlled by Saul, which would lead to some of the biblical traditions that made David the successor of and usurper over Saul.

In previous papers⁴ it was often possible to make definite and clear judgments about the biblical text and whether or not it was likely to be historical. In this study, things have been more slippery and indefinite. Nevertheless, it still seems possible to make some evaluations that have a distinct probability:

Biblical Text Confirmed

- The name “Israel” (Merneptah), showing some sort of entity in existence.
- Existence of Philistines in the coastal plain.
- Long-term division between Judah and Israel.
- David the founder of a dynasty in Judah (Tel Dan).

Biblical Text Not Confirmed but May Be Right

- Some part of Israel having been in Egypt.
- Diverse origins of Israelites (cf. Ezek 16:3).
- Saul as the first real king of Israel.
- Jerusalem made the capital of David's territory (chiefdom, city state, or incipient state).
- Temple in Jerusalem built by Solomon.

4. Grabbe 2005a, 2007b.

Biblical Text Most Likely Wrong

- No large-scale emigration of foreigners from Egypt in the LB age.
- Most of those making up later Israel likely indigenous, not immigrants.
- No unified conquest of the land as pictured in Joshua.
- No evidence found for a magnificent kingdom for Solomon, in spite of some recent attempts to reconstruct a splendid city of Jerusalem in the 10th century.

The situation is much more complicated in the biblical texts for this period than those for the later periods investigated previously. The conclusions about the events described in the text are not as clear-cut as in later periods, but one can still make reasonable judgments and determine some sort of statistical probability. What strikes one is the extent to which some major events as depicted by the text seem to be completely or mostly unhistorical. Whole blocks of tradition look like the invention—or at best the imperfect memory—of a much later time. Yet the author/compiler has not been completely bereft of information on the past: here and there we seem to find nuggets of reliable (or possibly reliable) data. These are blended not just with faulty memory but with non-historical aims: the construction of a theological, ideological treatise, among whose functions was national pride and ethnic identity. The impression gleaned from earlier studies is confirmed here: the tradition becomes more historically reliable the later in the monarchy one travels.

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HISTORY IN JOSHUA

Ernst Axel Knauf

Is Joshua a source for the LB–Iron transition? Instead of adding to the bulk of stated opinions on this particular topic, I would like to broaden the scope somewhat and ask: How much “history” found its way into the present (Hebrew) text of Joshua, and which kind of “history”? By “history” I mean, in the context of this particular essay, every element of the past real world that can be identified by a modern, historically informed reader in the text.

I accept the principal claims of the historical-critical approach to biblical literature as basically valid: that every book of the Bible is the product of a redactional process, starting with a first draft and leading gradually to its final form; that the principal stages of this development can be distinguished in the present text (at least roughly), having left enough traces and clues in it; and that they can be (sometimes roughly) dated if there is enough text for each stratum of the tradition, and if enough of the history presupposed by it is known. In opposition to past (and some present) scholarship, I do not look for individual authors or redactors, but assume that ancient Near Eastern and biblical literary production was a communal effort, one made synchronically and diachronically, by a group of scribes in the service of palaces, temples and their attached scribal schools (of which there were not too many in Israel, and even fewer in Judah).

The chronology presupposed in this study is the Low Chronology (Iron I–IIA transition between 950 and 900; Iron IIA–IIB transition between 850 and 800). Reconstructed features are classified as “probable” or “possible.” A feature termed “possible” is not less probable than a “probable” feature, as far as factuality is concerned, but its chronological window is wider than the date given here. Probabilities smaller than 0.5 will not be taken into account. Maybe “Sheshai” in Josh 15:14 serves as a monument to a Hyksos king (Redford 1967, 257)—but at least four other explanations of that name are as plausible, which leaves

for Redford’s proposal a probability of 0.2, or 20 percent. In addition, I will not even try to trace Joshua’s “memory” beyond the LB II period (at least not in the present study).

The literary history of Joshua, as I see it, may be outlined as follows:

late 7th cent.	First draft, end of an Exodus–Conquest Story (Josh 6* and 10*)
525–450	Exodus–Conquest Story becomes D-Composition (Josh 6–11*)
450–400	First Hexateuch Redaction (Josh 1–11*; 15:20–18:1*)
400–375	Second Hexateuch Redaction (Josh 13; 20–22; 24) and the Joshua Judges Redaction (Josh 12; 14; 18–19; 23)

I will start with a list (briefly annotated) of historical features that I found reflected in Joshua.

I. *The Data*

0. Joshua mentions kings for several Canaanite cities. Some of these were indeed seats of rulers (not necessarily “cities” in the LB II period); some were not. This item will be excluded from the data base, but used in the discussion. In the following table, 1 = probably existed, 0 = most probably did not exist.

City	Exod–Josh*	D	1Hexa	2Hexa	Josh–Judg
Jericho	0	0	0	0	0
Ai	-	0	0	0	0
Jerusalem	1	1	1	1	1
Hebron	0	1	1	1	1
Yarmut	0	0	0	0	0
Lachish	1	1	1	1	1
Eglon	0	0	0	0	0
Makedah	-	0	0	0	0
Libna	-	0	0	0	0
Gezer	-	1	1	1	1
Debir	-	0	0	0	0
Hazor	-	1	1	1	1
Madon	-	0	0	0	0
Shimron	-	1	1	1	1
Akshaf	-	1	1	1	1
Heshbon	-	-	-	0	0
Bashan	-	-	-	0	0
Geder	-	-	-	-	0
Horma	-	-	-	-	0
Arad	-	-	-	-	0
Adullam	-	-	-	-	0

Bethel	-	-	-	-	0
Tappuah	-	-	-	-	0
Hefer	-	-	-	-	0
Afek	-	-	-	-	1
Taanach	-	-	-	-	1
Megiddo	-	-	-	-	1
Kedesh	-	-	-	-	0
Jokneam	-	-	-	-	1
Dor	-	-	-	-	1
Gilgal	-	-	-	-	0
Tirzah	-	-	-	-	0
1/1+0 ratio	2/6 0.33	7/15 0.47	7/15 0.47	7/17 0.41	12/32 0.38

(a) There was a memory, not too vague but by no means very precise, that Canaan once was a country of city-states (*probable*). Instead of “memory” one might also think of an appropriate theoretical reconstruction by the scribes based on Canaan’s survival in Phoenicia and Philistia.

(b) Hazor was the most important of these states (*possible*)—Josh 11:10d. Instead of “memory,” the evidence of its large tell might have led to this insight, but it is nevertheless correct.

1. 13th century: Egyptian occupation of Canaan and Merneptah’s military street (*probable*), *ma’yan mē-neftoax*, “Spring-place of Mer-en-Ptah,” instead of “spring of the waters of Neftoah” (Josh 15:9; 18:15) (Singer 1988; Stager 1985).

2. 13th/12th century: survivors of the Israel annihilated by Mer-en-Ptah before 1208 B.C.E. join the new tribes filling its place on Mt. Ephraim and end up as a Manassite clan (*possible*): Asriel (Josh 17:2, 14) (Lemaire 1977; Whitelam 2000; Knauf 1994).

3. 10th century: (a) battle of Gibeon (*possible*)—Josh 10:1–14* (10:12c–13c). Gibeon (major town in Iron I) and Jerusalem were rivals for the control of the southern highway between the coastal plain and Transjordan in Iron I (this geographic feature lent prominence to Benjamin under Saul). Gibeon was probably allied with Saul’s Israel; Jerusalem most probably was not. So, the battle of Gibeon (the oldest source for which, though very fragmentary, was the polytheistic conjuration in Josh 10:12c–13c) probably took place during the wars between the House of Saul and the House of David, or shortly before. After Gibeon was destroyed by Shishak, it was not rebuilt by Solomon (Keel and Uehlinger 1994).

(b) When Jerusalem joined David’s kingdom, its native inhabitants still dominated the life of the town (wherever the term “Jebusite” came from, and whenever it was applied to them; *probable*)—Josh 15:63 (agrees with Ezek 16:3—except for the ethnonym—and the ethnicity of the victorious party in 1 Kgs 1, and which contradicts 2 Sam 2:6–8 which is, as a text, hopelessly garbled and no base for any historical reconstruction).

4. 10th/early 9th century: “The Book of the Upright” (*possible*)—quoted Josh 10:13d. The poetic fragments in Josh 10; 2 Sam 1 and 1 Kgs 8 are polytheistic and militaristic. They reflect the culture of courts of warlords (as does Judg 5). Iconography attests to a comparative mentality in Iron I and IIA. For David’s Lament I find an early-to-mid-9th-century origin most probable. As the “Book of Balaam” from Tell Deir ‘Alla shows, book production had started in 9th-century Israel (but these were, on the other hand, books quite different from those found in the Bible).¹

5. 10th/early 9th century: the Canaanite cities like Taanach and Megiddo are integrated into the state of Israel between Solomon/Jeroboam I and Omri (*probable*)—Josh 16:10; 17:11–13. Megiddo VI (10th century) is still indigenous Canaanite, VA still largely indigenous with intrusions of an extra-muros-state.

6. 8th century (reign of Jeroboam II): (a) the status of Taanach, Megiddo, Dor, Gezer and Edrei (*probable*)—Josh 17:11; 19:37. Cities in one tribal territory are garrisoned by recruits from another, a situation which matches Megiddo IV (75% “public buildings”).

(b) The fortresses of Naphtali (*probable*)—Josh 19:35–38. It was only in the 8th century that Hazor and Kinneret co-existed as fortified places (Knauf 2000).

(c) The borders of Zebulon (and perhaps Asher; *possible*)—Josh 19:10–14 (and 19:25–29*). These two border descriptions are not, like all the others, derived from Num 34:1–12, and rather precise. Dating based on 6(b)—data on Galilee seem to derive from Jeroboam’s II administration (also the only one that left some Hebrew inscriptions in Galilee).

(d) The town lists of Asher, Zebulon and Issachar (*possible*)—Josh 19:15, 17–21, 25–30*. By extension of 6(b).

1. Knauf 1997, 2002. Work on the 9th-century date of Tell Deir ‘Alla’s language is in progress; cf. preliminarily Knauf 2005.

(e) The administrative organization of Manasseh (*possible*)—Josh 17:1–6. Instead of districts with towns, Manasseh is organized in clans and their territories (also attested by the Samaria ostraca). These and the notion of an upsurge of Israelite tribalism/“nationalism” under (Jehu and) Jeroboam II suggest the dating followed here. Alternatively, one might think of deurbanization following the Assyrian conquest (and the *pax Assyriaca*), and attribute this feature to the 7th–5th centuries.

7. 7th century: (a) the territory of Dan (*possible*)—Josh 19:40–46. Looks like the territory of Ekron after 701 B.C.E. Was it part of Manasseh’s wish list after his participation in the Assyrian conquest of Egypt?

(b) The administrative organization of Judah and Benjamin in the second half of the 7th century (*probable*)—Josh 15:20–44, 48–62; 18:21–28. This describes Judah as a full-blown state (four-tier settlement hierarchy) and in the borders of, roughly, Josiah (Na’aman 1991).

(c) The first territorial concept of Eretz Israel (hereafter TCEI 1)—Josh 10:40–42. Judah in the borders of ca. 600 is presented as “the whole country,” that is, the terminal of the exodus: Eretz Israel = Judah. This is an accommodation of Judean state ideology to the newly acquired Benjaminites, to whom, for reasons of historical memory, the “House-of-David” story did not sound very attractive.

(d) The town list of Simeon (*possible*)—Josh 19:1–8. An excerpt from 7(b), but probably pre-exilic: Simeon = Ishmaelites who joined Judah (Arabs later immigrating and integrating into Judah became “Kaleb,” see *infra*).

8. 6th–early 5th centuries (525–450 B.C.E.): (a) TCEI 2 “from Dan to Beersheba” (*possible*)—Josh 11:16–23. Adding to 10:40–42, the concept of Eretz Israel is now brought in line with the territory of Israel and Judah as presupposed in Samuel–Kings. Belongs to a literary stratum which also presupposes D*, but not yet P. Dated by “sandwich” between 7(c) (prior to 597 B.C.E.) and P (later than 520 B.C.E.).

(b) “Clear forest” (*possible*)—Josh 17:14–18. After the Assyrian conquest, re-forestation in Galilee and Samaria was much more massive than in the LB period (no return to goat-herding, instead migration in search for labor). The conflict between Judah and Samaria might, however, as well be dated to the 4th century (Knauf 2003).

(c) “To hell with Bethel/Benjamin” (*possible*)—Josh 7–8 (notably 8:17; 12:16; the separation of 18:20–28 from 15:20–62). This anti-Bethel attitude was prevalent in Jerusalem from 520 to 450, but might well have lingered on into the 4th century (Knauf 2006).

9. Late 5th century: (a) TCEI 3 “Yehud and Samaria” (*probable*)—Josh 14:1–6*; 15:20–18:1. The sharp redactional cleavage between the land allotted to Judah and “Beth Joseph” and the land outside of Yehud and Samaria betrays a concept of Eretz Israel consisting of Judah and Samaria (together and in cooperation with the god of the Jewish diaspora, as illustrated by the Elephantine correspondence and the final redaction of the Torah). These are the politics and the ideology of the P group.

(b) “Make love—or covenants—not war” (*possible*)—Josh 2 and 9, showing Joshua to avoid slaughtering Canaanites under what pretext ever. The same politics as 9(a).

(c) Kaleb is Judah and is not Judah (*possible*)—Josh 14:6–15; 15:13–19. In 1 Chr 2 and 4, “Kaleb” is the genealogical anchor for Judean families of Edomite and/or Arab origin. This development seems to be prepared for by Kaleb’s treatment in Josh 14–15. The mechanisms of a tribal society (which allowed families and clans to move from one tribe to another, genealogies conveniently adapted) were very much alive in the Persian period. Idumaea—from a Jerusalemitic traditionalist point of view, southern Judah—was populated by Judeans, Arabs and Edomites in the 5th to 2nd centuries.

(d) “Solomon’s slaves” (Ezra 2:55 par.) become temples slaves (*possible*)—Josh 9. The Gibeonite towns, probably given to Solomon by Shishak, remained royal property throughout the monarchy. After the exile, the temple claimed the palace’s heritage.

10. Late 5th–early 4th centuries: (a) TCEI 4 “Nile to Sidon” (*probable*)—Josh 13 and most of the borders in Josh 15–19. A scholarly construct based on the merger of the Egyptian province of Canaan (reclaimed at least by Psammetikh and Nekho in the late 6th century, but possibly also by independent Egypt in the 4th century) with the Jordan as Israel’s border (from TCEI 1), and imported into Joshua from Num 34:1–12.

(b) “Resettle Galilee (and Transjordan)” (*probable*)—Josh 18:2–19:48. Prior to the Persian period, no Judeans lived in Galilee. In the middle of the 2nd century they are there and in Transjordan, parts of which were, together with Galilee, successfully claimed by Hyrcanus and his successors. Joshua 18–19 (together with Judg 17–18) seems to indicate the beginning of Jewish (as opposed to earlier Israelite) settlement in Galilee.

(c) Temples other than “of Jerusalem” (*probable*)—Josh 22, a theologically oriented instruction relating to the construction of a temple outside of Jerusalem, a problem posed by the temples on the Gerizim (since the first half of the 5th century), as well as at Maresha (5th/4th

century) and Elephantine. The “permit” granted to the Transjordanians in Josh 22 is more or less identical with the permit granted to the Jews of Elephantine in 408.

(d) Three Philistine cities (*possible*)—Josh 15:45–47. In the Persian period, only three out of five Philistine cities retained their semi-independence: Gaza, Ashkelon and Ashdod. Josh 15:45–47 obliterates its origin by naming Ashkelon “Ekron.”

(e) The Prominence of Qedesh Naphtali (*possible*)—Josh 12:22; 20:7; 21:32. Qedesh was the administrative center of Upper Galilee in the late Persian and early Hellenistic periods.

11. 3rd–2nd centuries: anti-Samaritan redaction (*probable*). The only Hellenistic influence on Joshua is found in its textual history. The book is pre-Hellenistic. A town list of Ephraim was deleted in Josh 16 (but partially quoted in Josh 21), and the venue of Josh 24 was changed from “Shiloh” (as in the LXX) to “Shechem,” changing the addressees of Joshua’s final speech from Judah and Samaria to Samaria only.

II. Discussion

The distribution of features from Joshua over the periods of Israel’s history (separated between *probable* and *possible*) turns out as follows:

<i>Period</i>	<i>Archaeological</i>	<i>Features (prob.)</i>	<i>Features (prob. + poss.)</i>
Pre-State	LB/Ir I	2	4
Early State	Ir II A	2	4
Independent State	Ir II B	2	5
Assyrian	Ir II C	2	4
Babylonian	Ir II C/P	-	-
Early Persian	P I	2	7
Late Persian	P II	3	5
Hellenistic	Hell	1	1
Sum		14	30

The picture that emerges becomes clearer if the periods are merged into even larger sections:

<i>Period</i>	<i>prob.</i>	<i>prob. and poss.</i>
Pre-State	2	4
Pre-Exilic	6	13
Persian	5	12
Hellenistic	1	1

In the “historical memory” of Joshua, the pre-state (“Settlement and Tribe” formation) and the Hellenistic periods are only marginally presented (the latter, because the book was basically finished before that period commenced). The pre-exilic (the period on which the biblical account from Genesis to 2 Kings is written) and the Persian periods (when that account was basically written, and mostly finished) feature with nearly equal weight. The slightly higher number of pre-exilic features should come as no surprise, for:

- (a) This history is much longer, four centuries as opposed to 1:5 (supposing that Joshua was finished ca. 375 B.C.E.);
- (b) There are also more data on most of the 10th to 7th centuries than for each of the 6th and 5th centuries, which might be used for the correlation of Joshua’s textual world with the real past world. If I knew more about the settlement history of Baal Gad and the town of Golan, I might have been able to add features for the Persian period.

The basic expectation that a literary text should reflect the time of its authors more faithfully than the time the authors write about is also fulfilled, for it is the later features which are devoted more text and more precision, as the following two examples demonstrate:

a. The Sequence of TCEIs

<i>TCEI</i>	<i>Date</i>	<i>Texts</i>	<i>Ex/implicit</i>
1	late 7th century	Josh 6*; 10*	implicit
2	6th/early 5th century	Josh 11	implicit
3	late 5th	Josh 15:20–18:1	implicit
4	late 5th/4th century	Josh 1:4; 13; 15:1–20, etc.	explicit

b. Egyptian Occupation, “Clear Forest,” and “Make Peace”

<i>Feature</i>	<i>Date</i>	<i>Number Words/ Verses/Chapters</i>	<i>Ratio</i>
Egyptian occupation	13th century	3 w. (1 place name)	
clear wood	5th century	80 w. / 4vv.	13th/5th: 0.04
make peace	late 5th century	55 vv. / 2 chs.	5th/15th: 0.07

The very first table (under I 0) shows, however, that the historicity rate of earlier literary strata is not necessarily higher than that of later strata. To look for a “core narrative” and then believe everything it says is not feasible; the same can be said of the disregarding of historical information because it is found in redactional additions.

There is a clear structure as far as the genre is concerned, a feature which transports historical memory. The earliest piece of historical consciousness is contained in one place name, mentioned twice for reasons which have nothing to do with its historical content, and which was opaque to the authors of the book and to those of their readers who were (or still are) unaware of the achievements of Pharaoh Merneptah.

The oldest piece of text quoted is from the 10th century. It is poetic, and committed into writing probably at the courts of Shechem, Tirzah, Samaria or and/or Jerusalem in the 10th to early 9th century. A prose composition from that time has survived only in epigraphy (TDA) and shows convincingly that we have nothing similar (except Judg 5) in the Bible. Larger amounts of text are preserved from the royal administrations of Israel and Judah in the 8th and 9th centuries, that is, at a time when Israelite and Judaean epigraphy sets in with considerable quantity.

This is an investigation of history in Joshua, not of the book of Joshua's constructing (wrong) history which is also done in the book. It would be interesting to contrast my lists of what the authors of Joshua knew (without knowing that they knew, in most cases) with a list of LB/Iron I history which they did not know. For them it was logical that anybody crossing the Jordan from the east in order to conquer Benjamin and Judah would first come to blows with Jericho. They needed the extinct tribe of Gad to organize areas of Jewish settlement in Peraea, so they rewrote its town list, found in Num 32:34–36 (which agrees with Mesha), in Josh 13 (which does not).

III. Conclusion

A quantitative approach to the "historical memory" of the book of Joshua helps to define the order of magnitude in which one may expect the survival of correct memory in biblical literature, and the production of history (as future memory) based on the experience of the authors present. We find both aspects present in Joshua, and where there are enough data (notably on social, economic, demographic, environmental and linguistic history), these activities can be identified and distinguished from each other.

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HISTORY IN JUDGES

Ernst Axel Knauf

Introduction

In the case of Joshua, basically a product of the early Persian period (500–400 B.C.E.), ca. 50 percent of the “historical memory” contained in the book comes from this period which might be defined as the book’s “present.” Another 25 percent derive from the 8th through 6th centuries, considerably less from the 10th and 9th centuries, and nearly nothing from the Late Bronze–Iron transition. For Judges, the “gestation period” is somewhat more extended.¹ It starts in the late 8th or early 7th centuries with the Israelite “Book of Saviours” (*Judg 3:9–12**) and does not end before the 3rd century (all dates are B.C.E.). The textual growth of Judges reminds of an onion. A pre-exilic core grew layers and layers in both directions, towards the beginning and towards the end. The “Book of Saviours” originated as a reaction to the loss of Israel’s king, kingship and independence. Foreign occupation—as now experienced by the hands of the Assyrians—is nothing new, it says. A saviour will appear—and hopefully not turn into a king, for kingship has failed, and was not such a good idea, after all. In an early re-issue, now including the story of Jephthah, it ends on a very negative, nearly hopeless note: not only the kings failed, the saviours failed too, in the end. Jephthah is buried in “one of the towns of Gilead” (*וַיָּקֶב בְּעִיר גִּלְעָד*, *Judg 12:7*)—no one cares which one precisely.

So far, what was to become Judges was an Israelite book, as was the first edition of Hosea (contemporary with the “Book of Saviours,” and addressing the same political situation, though proffering another solution: saviours are not really necessary, “prophetic” teacher will do). With the Persian period commences the Judaean adaptation, first by the addition of Samson. The anti-monarchic, anti-state attitude of the book is

now gradually turned around to pro-state propaganda. The beginning (1:1–3:11) and the end (chs. 17–21), both produced in the course of at least two redactions, were added to make the “book of Judges” fit its present context, Joshua and Samuel–Kings, in which it fills a “gap” that had not been there in the first place (in *Josh 18:1* the ark reaches the location where it is found at the onset of Samuel; in *Judg 1–19* the ark plays no role at all). The “book of the Saviours” became a “prophetic book” of the Hebrew Bible by means of a “prophetic redaction,” one which added, among other things, the story of Gideon’s “call,” “Baal fight” and sacrifice (6:11–32), which was not a very original achievement, for the whole story is nothing but Elijah recycled (*1 Kgs 17–19*).

The possibility of a pre-exilic macro-narrative from Joshua through Kings (“DtrH”) is excluded by the awkward redactional transition of *Josh 23:1–Judg 3:11²*—and by the length of the books of Joshua (656 verses) and Judges (618 verses). Only the book of Daniel (357 verses) is shorter. Had they ever formed a continuous narrative, there would never have been a sufficient reason to separate them: with 1274 verses, Joshua–Judges would still be shorter than Numbers (1288 verses), Isaiah (1291 verses), Jeremiah (1364 verses), Samuel (1506), Genesis and Kings (1534 verses) and Chronicles (1765 verses). The reason why Joshua and Judges form two separate books now is that they did so from their very beginnings.

A Brief Historical Reading

The following is a list of those sections of Judges for which a historical background comes to mind—which does not necessarily imply that the text is “based” on that particular event; the text might still be pure fantasy and any similarity between text and event a matter of pure chance. The possibility of a historical background within the known history of the 1st millennium B.C.E. does, however, diminish the probability of the text’s referring to otherwise unknown events or characters of the 2nd millennium.

1. *Judges 1:22–26*, a text from the latest literary stratum (late 4th/3rd centuries), reads:

The house of Joseph also went up against Bethel; and the LORD was with them. The house of Joseph sent out spies to Bethel (the name of the city was formerly Luz). When the spies saw a man coming out of the city, they said to him, “Show us the way into the city, and we will deal kindly

2. Blum (1997) aptly describes the Gordian magnitude of this knot, but did not have Alexander’s sword at hand to deal with it properly.

1. Guillaume (2004) more than compensates for the absence of an up-to-date commentary; cf. further Schmid 2008, 120.

with you." So he showed them the way into the city; and they put the city to the sword, but they let the man and all his family go. So the man went to the land of the Hittites and built a city, and named it Luz; that is its name to this day.

In the Samaritan tradition, the Gerizim temple is called "Bethel" and the settlement on the mount next to the temple, which flourished well into the Byzantine period, Luza. According to recent archaeological data, the temple was founded in the first half of the 5th century, that is, more or less immediately after the destruction of the Bethel sanctuary by Judean deuteronomistic zealots.³ The "land of the Hittites" in Persian period cuneiform texts is Syria. Without any doubt, the Gerizim is situated in (Greater) Syria, outside the borders of Yehud/Judea.

2. Judges 3:12–30, a text of the late 8th/early 7th centuries, contains three datable features:

(a) "Ehud (a Benjaminite clan) hit, struck, beat, killed, annihilated Eglon (a town, village or clan in northern Transjordan)" (cf. Knauf 1991) is an action perfectly feasible in the Near East when central state power is (temporarily) absent or very weak (cf. Mishqaq 1988). This was the case from ca. 1100 B.C.E. (it is unlikely that the tribe of Benjamin consolidated before the end of the 12th century) to ca. 875 (the end of the civil war which marked the beginning of Omride reign) and then again 724–716 (from the capture of the last Israelite king by the Assyrians to the organization of the province of Samaria by Sargon II).⁴

(b) The concept of a Moabite occupation of Jericho⁵ presupposes that the opposite side of the Jordan had become the מִזְרָחַת מוֹאָב, the section of the Rift Valley belonging to Moab, which was the case following Mesha's conquest of the Rubenite town of Nebo 850/840 B.C.E., but not before.

(c) Ehud escapes from a room locked from the inside by means of Assyrian type plumbing,⁶ which was not yet attested in the palaces of

3. Cf. Stern and Magen 2002; Knauf 2006. For Zech 7:2, which shows the cult of Bethel still operative in 518 B.C.E., cf. now Willi-Plein 2007, 123f.

4. Cf., for the late 8th century, the Nimrud letter ND 2773; Mittmann 1973.

5. Actually, Judg 3:13, "In alliance with...[Moab] went and defeated Israel; and they took possession of the city of palms," presupposes the occupation of Thamar at the southern end of the Dead Sea, which can hardly antedate the formation of the Moabite state under Mesha.

6. Cf. my commentary *ad locum*, forthcoming in *Neue Zürcher Bibel: Kommentierte Ausgabe*. פֶּרְשָׁדָ�נָה in 3:22 is the "canalization"; מְסֻדָּרָ�נָה in 3:23 is the place in the basement where it led to; and אַטְמָרָה in 3:15 is not "left-handed" but "thin, nimble, flexible" (cf. אַטְרִיוֹת, "spaghetti").

Samaria, but which is still visible today in the Assyrian palaces of Megiddo (III); this feature presupposes the establishment of Samaria and Megiddo as provincial capitals from 716 B.C.E. onwards.

3. Deborah, Barak and Sisera in Judg 4–5. The prose (from the late 8th/early 7th centuries) does not contain historical data other than those derived from the song, even though the song is secondarily inserted in the present literary context. The song was first committed into writing at the Omride court (875–850), and not composed before the emergence in the 10th century of "Israel" according to Judg 5, which leads geographically to the period between Eshbaal and Jeroboam I: Saul's Israel did not yet comprise the Galilee, and by the 9th century, Gad had replaced Reuben. Except for geography, social structure, and ideology, the song does not contain more "memory" than that of a conflict between the tribes of Zebulon and Naphtali and a sea-peoples chieftain or kinglet called Sisera. All other features might be drawn from type-scenes belonging to the stock of trade goods of a professional singer of that time.⁷

4. The Gideon story Judg 6–8 contains a basic text from the late 8th/early 7th century, its "source" in 8:4–27* added secondarily (but probably very soon, if not immediately together with the "elaborated story" of Gideon's victory in 7:1–8:3, as was the case with Judg 5 in respect to Judg 4), and several later additions. Who or when and where "Gideon" was, is impossible to decide, for the name seems to be derived from a ditty⁸ in which it might denote anything or nothing at all:

Look at me, and do the same; when I come to the outskirts of the camp,
do as I do. When I blow the trumpet, I and all who are with me, then you
also blow the trumpets around the whole camp, and shout, "For the Lord
and for Gideon!" (Judg 7:17–18)

This might be compared a ditty I was taught in the late 1950s by my maternal grandmother, to be said when a ladybird landed on one's hand:

Marienkäfer flieg
Vater ist im Krieg
Mutter ist im Pommernland
Pommernland ist abgebrannt [...]
Marienkäfer flieg

The text as I know it sounds incomplete, and it is to be noted that a variant circulates with *Maikäfer* instead of *Marienkäfer*. Devastating

7. Cf. Knauf 2005; Guillaume 2000.

8. The term "ditty" is apt given the assonance present in the Hebrew text.

wars in Pomerania took place in 1624–48, 1672–79, 1709–14, 1757–62, and 1806. The tradition most probably traces back to the 17th or 18th century C.E.⁹

(a) “Midianite” Bedouin—Judg 6:2–6. This text, which dates from the 6th/5th century, depicts the state of affairs at Benjamin’s southern border between 582 and ca. 525/450 (cf. Lam 5:9). Bedouin incursions so far west/north were previously impossible as long as Jericho, Jerusalem and the northern Shephelah were not destroyed and (largely) depopulated (cf. Blenkinsopp 2000).

(b) “Baal-fighter” Gideon—Judg 6:25–32. Dating from the 4th/3rd century(?), this text is based on “Elijah’s fight against Baal,” which reflects the fight of the Jerusalem monotheists against the Benjaminite traditionalists in the first half of the 5th century. The fight ended when the Samaritans founded their sanctuary on Mt. Gerizim, which was equally monotheistic and aniconic. The god “Baal” (in the singular, without further specification) is a theological construct produced to fit that very fight. He is not the deity attested at Ugarit (where the god addressed as “Lord” bears the proper name Haddu).¹⁰

(c) Gideon’s eastern campaign—Judg 8:3–21. This is a text from the late 8th/early 7th century telling how Midianites were involved in the Rift Valley copper trade from the 11th through the 9th centuries (Low Chronology). A conflict with the clan of Abiezer (which cannot have become a “Manassite” clan prior to the civil war under Omri) and a Midianite trading party is conceivable during the whole period. “Zalmunna” is a Taymanite name (“Salm protected”), and Tayma has now safely turned out to have belonged to the “Midianite” cultural sphere during the transition from the LB to the Iron Age.¹¹

(d) A campaign which destroyed Peniel and Succoth—Judg 8:13–17. This campaign is known to have been conducted by Shishak, in the aftermath of which Jeroboam I rebuilt Peniel.¹²

(e) The battle fought by Israelites at Qarqor/Qarqar—Judg 8:10. This battle took place in 853, in which also Arabs participated (if on the Israelite side). All three traditions are predominantly anchored in the 10th/9th century, and the amount of transformation—due to the lack of detailed sources (“annals”) for that period in the hands of the 7th-century scribes—is considerable.

9. Cf. for the redactional criticism of Gideon in Knauf 1988; Auld 1989.

10. Cf. n. 3 and Davies 2007.

11. Cf. Parr 1993; and now Hausleitner 2007; Finkelstein and Piasezky 2008.

12. Cf. for my interpretation of the Karnak inscription in Knauf 2000 (notably the map on p. 35); cf. Knauf 2007a.

5. Abimelech—Judg 9. The text is from the late 8th/early 7th centuries, and the final chapter of the original composition. Shechem was not settled from the late 11th through the late 10th centuries (according to the Low Chronology). In the “Rift Valley Economy,” the northern Central mountains were marginal (as opposed to the Benjamin region and Galilee). Shechem did not participate in the “Canaanite revival” of the 10th century, it was resettled as an Israelite royal residence. Under these circumstances, it is most unlikely that a tradition predating ca. 1050 B.C.E. could have reached the Hebrew Bible. Würthwein regarded Judg 9 as a pure literary construct without a factual base (Würthwein 1994). As an archetypical treatise on “tribal states” and how they fail due to insurmountable tensions between tribe(s) and town(s), “Abimelech and Shechem” might well reflect “Saul and Gibeon” as well as “David and Jerusalem” (cf. Knauf 2007b).

6. Jephthah’s victory—Judg 11:33. A text from the 6th century, what is described with the words “from Aroer as far as Minnith, all the way to Abel-Keramim” is the southern border of the Ammonites, from Tell ‘Umeirī to Sahāb in the east. Aroer—there is only one in Transjordan—has always been a Moabite town (cf. Knauf 1992, 1998). So the aggressor came from the south; whether he was a king of Moab or the Babylonian army in 582 B.C.E., we cannot know, but the latter date is, at least, not hypothetical.

7. (a) Samson—Judg 13–16. This is a text from the 6th/5th centuries which reflects Judaean–Philistine rivalry on the village level, without any state authority interfering with Samsonite shenanigans. The presupposed political situation coincides with the supposed time of composition. Prior to David, Philistia did not have a “Judaean” neighbour, and vice versa. On the other hand, parallels to Jephthah’s daughter and to Samson’s pranks do not necessitate a Hellenistic date for these stories. Greeks and Hebrews share a common cultural background in the Bronze Age Levant, “Greek” themes and motives could have been imported and disseminated by the Sea People, and the penetration of the coastal region by Greek mercenaries and specialists starts in the 7th century and gains more momentum in the 5th and 4th centuries.

(b) The place-name “Mahaneh Dan”—Judg 13:25; 18:12. These are texts from the 6th through 4th century that memorize the immigration of the sea people (Danuna) in the 12th/11th century. This event was re-interpreted (at its second occurrence!) in the biblical account as a stopping point on the fictional migration of one of Israel’s more fictional tribes.

8. The migration of the Danites—Judg 17–18. A text from the 4th century, this account reflects Judaean colonization of Galilee, which cannot have started later than the 4th century B.C.E. There were no Jews in Galilee prior to the 6th century, but from the late 2nd century B.C.E. onwards, there were many.

9. The Mizpah affair—Judg 19–21. This text, from the 4th/3rd century, reflects Judaean–Benjamite rivalry between 525 and 450, which culminated in the destruction of the sanctuary of Bethel by the Returnees,¹³ and the shift of provincial capital back from Mizpah to Jerusalem in 445/44. The victors are telling the defeated that it all was their fault in the first place.¹⁴

Conclusions

1. The 200-year span of “the past” in collective memory has re-emerged. In a story-telling community of grandparents, parents, children and grand-children, the “present” lasts ca. 100 years. (My own experience bears this out: I “know,” by means of family tradition rather than books or historical documents, of events dating back as far as the beginning of the 20th century C.E.) The 100–200 years before the “present” are considered “past,” though in a sense this is past is still alive, if somewhat vaguely. (Another personal experience: my grandmother still “knew” of a relative who had been a Prussian admiral in the middle of the 18th century. While this person certainly existed, he had been, in fact, commander-in-chief of the [rather unimpressive] Prussian fleet, with the rank of captain rather than admiral.) Everything before is “distant past,” another world, where donkeys see angels and talk to prophets.

2. In the table opposite the data offered above are summarized. “Probable” temporal distance has been marked by “x,” while the siglum “?” is used to mark a “possible” distance. In the “probable” range, 5/11 features fall into the “100 years present” (45.45%), 3/11 into the “200 years past” (27.27%), and another three into the “distant past.” In the “possible” range, 5/12 features belong to the “200 years past” (41.67%), 4/12 to the “distant past” (33.33%), and only 3/12 features to the “present” (25%).

<i>Text</i>	<i>Date of Composition</i>	<i>Date of the “Event”</i>	<i>Less than 100 years</i>	<i>Less than 200 years</i>	<i>More than 200 years</i>
1:22–26	325–250	500–450	x	?	?
3:12–30: Ehud vs. Eglon	700–650	1100–875; 724–716	?	?	?
3:12–30: Abbot Moab	700–650	850–650	x		
3:12–30: Assyrian plumbing	700–650	716ff	x		
5:2–30	875–850	975–900	x		
6:2–5	500–250	582–520	?	?	?
6:11–32	325–250	500–450	x		
8:4–28: Midian and Taya'na	700–650	1150–825	?	?	?
8:4–28: Succoth and Pennel	700–650	940–910	x		
8:4–28: Karkor	700–650	854	x		
9	700–650	1000–950	x		
11:33	582–500	582?	?	?	?
13–16	600–450	600–450	x		
13:25; 18:12: Mahaneh Dan	600–300	1150–1050	x		
17–18	400–300	400ff.	x		
19–21	400–250	500–450	?	?	?

13. Cf. Judg 1:22–26 and n. 3.

14. Cf. Guillaume 2004, 198–226; also Amit 2006.

Secondly, discontinuity on the level of the elites, as occurred several times in Israel between 1000 and 720, results in breaks in the historical tradition. “Popular” tradition has preserved some memory of things happening at Qarqar, Penuel and Succoth, but re-attributed these “labels” rather freely. The “silencing of Omride history” is most notable; of the rich literature which once must have existed, only the Song of Deborah survives (and, in fragments, and not from the biblical tradition, the Bileam inscription from Tell Deir ‘Alla).

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HOW TO DEAL WITH "EARLY ISRAEL"

Niels Peter Lemche

Sometimes you know a subject too well, and see no more reason to proceed with it. Maybe nothing has happened since you left the field, sometimes what happened is not very interesting, or even represents a step backward.

When Lester Grabbe proposed the subject of early Israel for this seminar two years ago, some of the participants, including me and Philip Davies, argued against it. We couldn't see much happening that allowed for a new vista on the subject.

A number of issues are involved:

1. What happened in the way of writing a history of the period?
2. What happened from an archaeological point of view?
3. What happened within social anthropology that would help reconstruct at least an outline of the history of Israel in the early phase of its history?
4. What happened in the way of reading biblical texts that would allow for new ideas about history and historicity?
5. What kind of history can be written?

As far as the first question is concerned—What happened in the way of writing a history of the period?—the answer might be: Not much. A monograph or two and some chapters in general histories of ancient Israel.

As far as the second issue is concerned: What happened from an archaeological point of view? The answer could be: Not much that goes beyond the Tel Aviv archaeologists' territorial surveys of the 1980s and 1990s.

As far as the third issue is concerned—What happened within social anthropology that would help reconstruct at least an outline of the history of Israel in the early phase of its history?—the answer might be: Not much, apart from some retrograde studies trying to introduce obsolete anthropology.

As far as the fourth question is concerned—What happened in the way of reading biblical texts that would allow for new ideas about history and historicity?—the answer might be: In the strict sense, referring to the quest for the historicity of events mentioned in these texts, not much happened, but in a broader sense referring to the reading of biblical texts, quite a lot has happened. However, the situation is not very promising for the extraction of historical information from biblical texts.

As far as the fifth question is concerned—What kind of history can be written?—the answer might have a "prophetic" flair: Such a history should be written without the Old Testament. The Old Testament only distorts the interpretation of whatever information we have from the Early Iron Age.

Having given brief responses to the five questions, it is appropriate to explore more fully the answers given:

1. *What happened in the way of writing a history of the period?*

In the last few years two major histories of Israel have appeared, one by Liverani, *Israel's History and the History of Israel* (2005), and the second extensively rewritten and enlarged *A History of Ancient Israel and Judah* by J. Maxwell Miller and John H. Hayes (2006). If we concentrate for a moment on the latter title, it is obvious that Max Miller is faced with some of the same problems that became obvious in J. Alberto Soggin's earlier *A History of Israel: From the Beginnings to the Bar Kochba Revolt, AD 135* (1985) as new editions were published. Already the second English edition had a very important change of title: *An Introduction to the History of Israel and Judah* (1993) (I haven't seen the third edition, which is only published in Italian: *Storia d'Israele. Introduzione alla storia d'Israele e Giuda dalle origini alla rivolta di Bar Kochba* [2002]). The problem is clearly that the subject is slipping between the fingers of the researcher. In their preface, Miller and Hayes make a very important point: "During the 1980s...it was necessary to devote considerable space to explaining why the biblical presentations of Israel's origins could not be taken even essentially at face value. Today it is equally necessary to make a case for utilizing the biblical materials at all in historical research" (Miller and Hayes 2006, xvii). The critics of traditional history writing from, say, Rudolf Kittel and Ernst Sellin to Herbert Donner are victorious to such a degree that, with the exception of the evangelical ones, including Ian Provan, V. Philips Long, and Tremper Longman III (2003), the majority of contemporary biblical scholars have simply left the field of historical studies. (Jens B. Kofoed's 2005 defence, being part of an evangelical defence of historicity [cf. also

V. Philips Long, David W. Baker, and Gordon J. Wenham 2002] is of no avail.) History has, as Leo G. Perdue argues, “collapsed.” This is a fundamental observation, as evidenced in his two monographs on the theme, *The Collapse of History: Reconstructing Old Testament Theology* (1994), and *Reconstructing Old Testament Theology: After the Collapse of History* (2005)—again, a very significant change of title.

Perdue’s attitude will of course be very different from the ones entertained by members of the ESHM, in that he does not really discuss historical reconstruction, but focuses rather on the importance of history in modern societies. His is about respect and authority: nobody pays attention to history anymore. It is a pastime of the past. Whether or not something really happened is of no consequence today. In the present study I will agree and also heartily disagree. History is just as important as the importance placed in history in the mind of people. After all, the *Da Vinci Code* must tell all doubters that the general interest in history—although sometimes somewhat perverse—is unabated. Maybe it is, after all, mostly among so-called sophisticated intellectuals that the interest in history has disappeared. Even if there are no substantial questions involved, historical reconstruction is still extremely popular reading, and historical novelists make ample use of this interest in the past. But what happens when problems arise? When Thomas Thompson’s 1999 monograph was published, the word “myth” in US title was translated by the public as “lie.” It is not coincidental that the US title, *The Mythic Past: Biblical Archaeology and the Myth of Israel*, was replaced in the British printing with a more “neutral” title, *The Bible in History: How Writers Create a Past*.

History collapses not because of a few intellectuals who leave the field but because of itself—in this case because an ancient historical source as remarkable as the Bible is not about *history* at all. When it is a “lie,” nobody is interested anymore, as William G. Dever has realized.¹ The subtle distinction between different “realities” is way over the heads of most people of the modern age, including the journalists who try to communicate developments within scholarship to the lay.

Miller and Hayes’s quest for legitimacy of historical studies is certainly a serious issue. And when it comes to the history of Israel the questions are: Did it really happen? And why should this be important today? History was to the earlier generation of von Rad and Noth in the centre of interest, and this involved also their theological interpretation.²

1. Among his many tirades against the minimalists, see Dever 2001, 2–3.

2. Martin Noth only sporadically touched on theological issues. An exception is his “Die Vergegenwärtigung des Alten Testaments in der Verkündigung” (1960).

However, in contemporary theology, it is about the past, and theology mostly has to do with the present and the future (not to speak of some of the basic assumptions of the time, e.g., the assumption that ancient Israel was something special and its religion absolutely different from that of the surrounding countries, which have been shown to be false).

Miller and Hayes’s work, in its second edition, is a welcome “middle-of-the-road” assessment of the present state of historical studies. Of course, whether it remains a textbook in contemporary courses introducing new students to the biblical “world” will say a lot about its standing. Alas, as I have stated many times, in print and directly to Israel Finkelstein, the middle of the road is a dangerous place to be.³ Not only does the position advance nothing, but it fails to satisfy any part fully. Nothing happens here. Self-preservation seems the obvious stratagem. But, worse, it is a position under fire from both sides. I remember Dever also trying to position himself in the middle and being attacked from the right by David W. Baker and Bill T. Arnold, who referred to the discussion that took place at the SBL National Meeting in New Orleans in 2006 between this writer, Thomas Thompson, P. Kyle McCarter and Bill Dever in the following way: “...the conversation, ‘Face to Face: Biblical Minimalists Meet their Challengers,’ where N. P. Lemche, T. L. Thompson, W. Dever, and P. Kyle McCarter Jr. expound[ed] their minimalist views” (Baker and Arnold 1999, 80 n. 97; see Shanks 1997 for a treatment of the discussion). In the eyes of a truly conservative scholar, then, everything to the left of the far right is “minimalist.” Of course, true radical scholars will never accept a position in the middle, and will often consider it more or less a repetition of the bad ways of the past. Thus Thomas Thompson is very outspoken in his criticism of Finkelstein’s (and Neil Silberman’s) position in a recent review: “Finkelstein and Silberman’s book is judged as an unsuccessful attempt to return to the methods of ‘biblical archaeology’ that were legitimately impeached in the mid-1970s” (Thompson 2005). I am not sure that Israel Finkelstein will be happy to accept this criticism. I need not add what evangelicals would say.

The theology of his contemporary, Gerhard von Rad (1957–60, ET 1962–65), is more a kind of *Mentalitätsgeschichte* than a theology in the normative sense, based as it is on a tradition history which is again founded on the reconstruction of Israel’s history produced by the German tradition of historical investigation, which was well known from Noth (1950, ET 1958).

3. Cf., for example, Israel Finkelstein’s repeated defence of his position as “in the middle”; see Finkelstein and Silberman 2001, 2006.

Although Miller and Hayes's volume will certainly remain influential for a while—perhaps because it really does not provoke dissent from the majority of people in the field—it will produce little in the way of new knowledge.

The second volume on the history of Israel, by Mario Liverani, is more innovative. I will return to it later.

The three more specialized monographs I wish to discuss, by Robert D. Miller II, Ann E. Killebrew, and Stephen L. Cook, are mostly based on a social-anthropological discourse and so will be discussed below in connection with that subject.

2. What happened from an archaeological point of view?

Extensive archaeological excavations have been conducted, but few of the more important ones are directly pertinent to the issue of early Israelite history, that is, the 13th through 10th centuries. Or rather, few have changed much. Generally, the Tel Aviv way of reconstructing the archaeological view of the Early Iron Age (or better: the transitional period) has met little challenge—apart from by scholars at the Hebrew University. We are a far cry away the earlier politically inspired biblical archaeology as attacked in some modern literature, including Nadia Abu el-Haj's *Facts on the Ground: Archaeological Practice and Territorial Self-Fashioning in Israeli Society* (2001), which earned her little credit but made her a victim, I suppose, of the Campus Watch organization;⁴ Raz Kletter's *Just Past? The Making of Israeli Archaeology* (2006), which, I imagine, didn't help the promotion of his career; and Terje Østigård's pamphlet, *Political Archaeology and Holy Nationalism: Archaeological Battles Over the Bible and Land in Israel and Palestine from 1967–2000* (2007). Østigård, who by the way is an archaeologist who has dug extensively in different parts of the world but never in Israel/Palestine, is particularly offset by the use of ethnicity criteria in biblical archaeology as well as its naïve identification of artifacts with special ethnic groups such as the Israelites and the Canaanites.

There is in itself nothing upsetting about using history, including archaeology, to boost nationalism. History originated because of the need for national history following the creation of European national states after the French revolution. I have dealt extensively with this elsewhere (particularly in *The Israelite in History and Tradition* [1998, 1–21]), and there is no need to repeat the argument here. That the creators of the modern state of Israel, mostly European by origin, and children of the late 19th century, choose history as a vehicle of Jewish nationalism,

changing Judaism from a religion to a political organization, is self-evident. Nor is it strange that the former Marxists involved in the 1968 rebellions chose history as one of their primary hate subjects. By destroying history, they believed also in the name of internationalism to have destroyed nationality. This was done by removing history as a subject in primary schools. If you don't teach the children who they are, they will end up not knowing who they are, and easy victims of nationalist propaganda. To some degree they succeeded, but only to some degree, as the interest in the past is still very much in evidence, though perhaps not as controlled by facts as it used to be.

In Israel, the Prime Minister Ben Gurion assembled a number of archaeologists and historians around him, including a respected historian like Abraham Malamat. Ben Gurion understood perfectly the importance of having the *right* (not necessarily the correct) history. He would probably not have objected too strongly to the modern idea that this is not history; it is story—albeit a good story, that is, one promoting national sentiments (or he might after all himself have believed in this story as history). Of course, it is a problem to historical and archaeological research in the professional sense of the words that, as in this case, such strong nationalistic sentiments are involved—and so far we have not yet considered religious sentiments!

It is, on the other side, not so recommendable that archaeology in Eretz Israel for a very long time was (and I hope is not anymore) influenced by ideas of ethnicity and archaeology fostered in Europe by Gustav Kossinna (1858–1931), Heinrich Himmler's favourite archaeological authority on the origins of the Germans (Kossina 1911). This is the special complaint brought forward by Østigård (2007, 40–47) in his attack on William G. Dever. However, it is a (sad?) fact that we learn much more from our enemies than from our friends. Himmler's intellectual background (if I am allowed to use such a term in this connection) was Germany at the beginning of the 20th century building on its spiritual forefathers, especially Ernst Moritz Arndt and his ideas of the chosen people, in his case the German people. However, the intellectual background of some of the major figures in modern Israeli history were just as well adhering to the same ideology, although of course centring around another identity, the one of Israel as opposed to its (modern) neighbours. I cannot tell if every archaeologist of the present time has freed him- or herself from this ideological muddle; I can only hope it. I suppose that most archaeologists in Europe have got so far.

The problem about identity has to do with the concept of ethnicity. I shall return to this in the next paragraph. So far I can only repeat myself and say that archaeology has for the last ten years not changed much, and

4. Cf. <http://www.campus-watch.org/>.

that the general layout of archaeology which was proposed by Israel Finkelstein twenty years ago is still basically unchallenged (cf. Finkelstein 1988). The only major breakthrough, as I read archaeology, does not concern early Israel (at least if you don't adopt a "Copenhagener" view according to which everything predating the second century [B.]C.E. is considered "early"), but the 6th, 5th, and 4th centuries in the shape of Oded Lipschits's Finkelstein-like territorial surveys.⁵ The soundness of Finkelstein's approach is confirmed by the likewise happy application of the methodology by Lipschits.

3. What happened within social anthropology that would help reconstructing at least an outline of the history of Israel in the early phase of its history?

Social anthropology is an enormous field of studies with a series of remarkable directions, much bigger than anything associated with biblical studies. After all, it has the whole world as its subject. A recent review of the field by three "grand old men" and one "grand old woman"—Fredrik Barth, André Gingrich, Robert Parkin, and Sydel Silverman (2005)—divides the field into four main directions, labelled as British, German, French, and American anthropology. It would be arrogant to dismiss, say, the studies of Claude Levi-Strauss as of no importance to biblical studies. Although we don't read him anymore, as a young Danish anthropologist claimed a few years ago when asked about the importance of this French anthropologist, his four volumes of *Mythologiques* (1964–71) and his extremely influential *La pensée sauvage* (1964) remain classics and indispensable. However, it is neither the British nor the French schools of anthropology (and not a word about the German variety) which have had an influence on biblical studies—rather, it is the North American division of anthropology, *Cultural Evolutionism*, or *Cultural Anthropology*, that conquered the scene, although on premises which are, shall we say, flawed. Again, I have dealt with this in several publications, of course extensively in *Early Israel* (1985), but also in my article in *SJOT* on "System Theory" (1990).

Cultural Evolutionism is a fearful creature, enticing people, luring them to believe in something about which we have no idea (or too many ideas), such as the development of humankind. History books have been written on this basis, and archaeological theory developed. And yet, it is all questionable. When I wrote the first draft of *Early Israel*, I was very much enticed by the spells of this school. Happily, my wife, with a

5. Lipschits 2005, supplemented by Lipschits 2003, 2006, and Lipschits and Tal 2007.

degree in social anthropology (under the supervision of Johannes Nicolaisen [1921–1980], a Danish expert on nomadism, who worked extensively among the Tuaregs),⁶ warned me against it, and persuaded me to follow a more field-related path, relying on field reports and not secondary "arm-chair" theory.

At the time, when I wrote that book, Norman Gottwald's *The Tribes of Yahweh* (1979), with its mixture of Cultural Evolutionism and Marxist anthropological theory, was dominating the field. I assume that the appearance of *Early Israel* put an end to that, and it is therefore sad to see the mistakes of the past reappearing in recent literature, as in Stephen L. Cook's *The Social Roots of Biblical Yahwism* (2004), a study borrowing heavily from Gottwald, and for the same reason difficult to be very serious about.

[As an aside, it is strange to see how often biblical scholars adopt anthropological theory formed by people not even quoted in anthropological textbooks or reviews, such as the aforementioned by Barth et al. Biblical scholars repeatedly commit the sin of incorporating work from another field and applying it to their own subject without appreciating the status of their extracurricular reading material within its own field.]

Now, Cook's book has to do with the development of Yahwism in the period of the Hebrew Kings, and not early Israel. There is therefore no reason to go further with this title. Instead, I will turn for a moment to another study of the transitional period by Ann Killebrew, *Biblical Peoples and Ethnicity: An Archaeological Study of Egyptians, Canaanites, Philistines, and Early Israel, 1300–1100 B.C.E.* (2005), which is, as the curate's egg, good in parts—that is, the archaeology may be fine (or my archaeological colleagues will have to explain why not), but the understanding of the basic concept of ethnicity is certainly problematic. I reviewed her book at the SBL Meeting in Edinburgh in 2006. Here just a quote from that paper: "it is a little sad to see Ann Killebrew in her new study on the Ethnicity in the Iron Age referring to Michael Rowton's old studies on the dimorphic society, as Fredrik Barth and following him this speaker has clearly shown that Rowton's basic idea of the competition between nomads and the settled population is misleading."

The reason is obvious: she has not read Barth, or any other anthropological studies about nomadism in the Middle East. The only work by Barth included in her bibliography is his introduction to *Ethnic Groups and Boundaries* (1969), which is of course compulsory reading for anyone who wishes to enter into the discussion of ethnicity. There are, however, several other more recent studies on ethnicity. I made a survey

6. Cf. Nicolaisen 1963.

of the then *status quaestionis* in *The Israelites in History and Tradition* (Lemche 1998, 8–20), and Østigård has a fine discussion in his treatment of biblical archaeology (2007, 30–36). There seems to be a special Norwegian emphasis on the subject, as witnessed not only in Barth's studies, but also in the publications of one of Barth's students, Thomas Hylland Eriksen (1993a, 1993b).

In an article on the Philistines, the Danish archaeologist John Strange presented four criteria valid for the demonstration of Philistine ethnicity (Strange 1998):

1. Contemporary texts, or texts beyond historical dispute, mentioning the country of origin from where this ethnos migrated.
2. A well-defined cumulative archaeological material (one type, e.g., pottery, is insufficient).
3. The same or similar material must be found in the country of origin.
4. A certain, relatively fast acculturation.

Why is some of this problematic? Because, as has been stated by Barth and his fellow anthropologists over and over again, material culture does not necessarily conform to ethnicity. The same material culture does not necessarily mean the same ethnicity. Østigård, in his attack on Dever, makes this a major weapon in his arsenal. He could just as well have taken Ann Killebrew to task. Simply because you understand a special material culture does not mean that you can necessarily translate that material culture into ethnic terms. When Dever is talking about "Proto-Israelites," Østigård has no problems showing that this term is meaningless, because it has been coined by biblical language. Why they are "proto" has solely to do with them not being biblical Israelites, but according to this criterion, no inhabitant in Palestine before, say, the Persian or Hellenistic times was "Israelite"; they were all "proto-Israelites."

Anthropology has much to offer, but it should be properly read and understood. Many times, when biblical scholars working with anthropology are met with distrust, it is simply because the critics have not done their anthropological homework. This also includes—because social anthropology is a branch of general sociology and therefore belongs among the behavioural sciences of the 20th century—language and ideology. Postmodernism would not have developed without these disciplines, but generally biblical historians have never read much of it because they prefer plain reading, simple explanations, and are opposed to twisted language, that is, language coloured by the behavioural sciences, including psychology and linguistics.

On top of this, Cultural Evolutionism is not dead. One of the more remarkable studies on early Israel that has appeared in the last few years, Robert D. Miller's *Chieftains of the Highland Clans: A History of Israel in the 12th and 11th Centuries B.C.* (2005), makes extensive use of models, or rather one such, that of "complex chiefdom." I reviewed the book in *SJOT* 2005 and have little more to say (Lemche 2005). Miller believes that his model allows him to trace two major chiefdoms in the mountains to the north of Jerusalem, one with Shechem as its centre, the other having Shiloh as its centre. There are many things to recommend this study, but also some problematic issues. His use of the concept of ethnicity is no better than Ann Killebrew's. He hasn't read Barth's essay on ethnicity—only his book about *The Nomads of South Persia* (1964). As far as archaeology goes, he sides with the Mazar "School" and rejects the Low Chronology of Israel Finkelstein and his Tel Aviv colleagues. In the end, my verdict was that he made a plausible defence for the model chosen, but also that this model is only one among several possible. The problem remains: this is not *A History of Israel in the 12th and 11th Centuries B.C.*—it is an attempt at showing how it might have been, and no more.

Summing up: The picture of the relations between social anthropology and biblical studies may have improved in some parts of the discipline of biblical studies. However, when it comes to the incorporation of sociological analysis into historical studies, not much has happened, and what has happened may represent steps backwards, rather than theoretical improvement.

4. What happened in the way of reading biblical texts that would allow for new ideas about history and historicity?

As perceived by Perdue and other scholars not interested in history, historical studies have produced nothing more than a divide between believers of all camps and biblical studies in general. This may not be a problem to the historian strictly speaking, writing for the benefit of his or her colleagues. It might be a problem for the survival of history in theological departments all over the Western World. Dever realized this very early on, as did also Hershel Shanks, who saw no interest in the archaeology of Israel/Palestine if it could not be linked to biblical issues and ultimately able to prove that the Bible is after all right. Dever's triumphant claim that biblical archaeology is dead was soon changed into a vehement defence of biblical archaeology.

It is quite interesting when we are taking the temperature of scholarship that such books as my *The Canaanites in History and Tradition* (1991) and Thomas Thompson's *The Bible in History* found much more

acceptance among literary students than among biblical historians. I still remember Alice Bach, a well-known figure within literary studies, exclaiming, having read *The Canaanites*: "Now I finally know why historical studies may be important." Historians are not very happy about ideological reading, not even such reading that explains the ideological framework of ancient circles. In their eyes this is twisted reading. It makes complicated what they think is not complicated at all. This was definitely the problem of Nadav Na'aman in his review of *The Canaanites*, a work which, in his eyes, was based on a "twisted" reading of biblical and ancient Near Eastern texts (Na'aman 1996).

Therefore any union between historical and literary studies had to be given up. Ultimately, history lost, and within a very few years became a subject for the minority. Of course, it might be said that the historians won the day; after all, many contemporary literary students' idea of history is based on an early reading of John Bright's history or something even more traditional or conservative. But this is hardly the critical historian's position. So, this has really to do with the status of hermeneutics. Postmodernism—and I will have to stress that the Copenhagen School is not postmodern, but modern, though it may help the promotion of postmodern ideas—based on insights from the behavioural sciences, has changed the subject under scrutiny from being something foreign (i.e. external) to the reader to personal to the reader. This is bad for classic history writing, because this change of focus underscores a truism in postmodernity: that the relationship between the reader and his/her subject is primary in contrast to the relationship between his/her subject and what it may reflect.

We see often in discussions about historicity and biblical issues how this argument is used and abused. Everything is relative. Scholars are individuals and their readings are personal ones. Who can decide which reading is "the best"? If I and, for example, Nadav Na'aman disagree on a subject, this is not part of the subject being discussed; it has to do with differences of mind and attitude between Na'aman and me. The "reader response" variety of literary study, which was initiated by a famous lecture—I believe in 1966—by James Muilenburg, has become endemic to much modern reading. For history and historical studies this is a deadly threat. If literary studies of this kind contribute to historical studies, it will be to the death of the discipline. It cannot, however, be ignored, and historians will have to establish a counterattack if they are going to survive.

This is a difficult task because today's *intellectual* mood—recall the distinction made above between intellectual understanding and lay

persons' reading—does not promote historical interest, meaning that in some parts of the world, say Europe, historical studies may only complicate the process, turning the Europeans into citizens of only one state. The European Union is the logical conclusion to a process that began in 1789. The period between 1789 and 1956—the year in which the Common Market, the forerunner of the European Union, came into being—was one long history of war, from the revolutionary and very bloody wars, via innumerable wars during the 19th century, to the two World wars of the 20th century. This history should in the eyes of many Europeans best be forgotten, or a new one be written stressing coherence and not discord.

The question "How do literary studies influence historical studies?" is important, but the answer may be rather negative. As far as influence is concerned, this impact is mostly negative. We don't need any history; history is a "scandal that never ends" as argued by Elsa Morante, an Italian novelist. We don't need more scandals, and accordingly we don't need history.

If literary studies have attributed something, and I insist that in spite of its reception among biblical historians, Thompson's *The Bible in History* may point in the right direction, it is by taking up the task of hermeneutical interpretation of biblical reading. Here a lot can be done. Thus it is to my "historian" mind clear—and I confess to not being a supporter of reader-response reading—that most of modern literary theory applied to biblical literature is misleading. The reason is simple: because of a total change of aesthetic norms since antiquity. As a musician once explained to me, the change came about in early Romanticism: until the late 18th century a piece of music was understood as a series of periods, times, or episodes, and not as a total whole. Since Romanticism, we understand a piece of art—a piece of music, a painting, or work of literature, or whatever—as a whole. The difference is very clear if we compare a composition by Bach to a symphony by Brahms.

In Antiquity, the *pesharim* from Qumran show a totally different way of understanding biblical texts from anything modern. Questions arise when, as in the New Testament, we find allegorical interpretations casting new light on what has been said. In the Gospel of Mark, for example, Mark has Jesus explaining one of his parables as an allegory (Mark 4:13–20). The question is: How did the ancient author write? The answer: Without any sense of, in this case, the possibility of an allegorical interpretation. Recently, a young German scholar, Ulrike Sals, has published a piece of work analyzing a medieval interpretation of the book of Numbers (Sals 2007). Her argument is that historical-critical

scholars find many problems when they read this biblical book, it being especially difficult for them to keep the book as a unified composition. In contrast, however, the medieval commentary to Numbers with which she deals shows no awareness that anything is problematic. The medieval scholar did not encounter historical-critical problems, which is natural, and certainly did not see any breaks and disunity. Are these problems something we caused by a modern and historical reading? Are we Europeans—as I argued in a lecture in Stellenbosch more than ten years ago (Lemche 1999)—really good readers of biblical texts and interpreters of biblical history? Can we escape modern hermeneutics and continue to be biblical scholars? I doubt it, even if it forces us to change direction, and to present “twisted” readings of biblical *and other ancient Near Eastern texts*. It is not my view, or that of Thomas Thompson, that this is impossible or avoidable. Neither do we think that it should be avoided. Ancient literature is not for plain reading. It is much more sophisticated—“twisted”—than that.

Ancient historiography becomes interesting when it is not so much about what happened as about what people believed to have happened. This might, in spite of all the odds, still be an interesting subject to study.

5. What kind of history can be written?

In 2003, Mario Liverani published his *Oltre la Bibbia: Storia antica di Israele*, a work subsequently translated into English and published (in 2005) as *Israel's History and the History of Israel*. Liverani had already some years before discussed the kind of historiography found among historical-critical scholars, scholars who had, in his eyes, only presented hypertexts based on biblical narrative (Liverani 1999). These historiographers had developed the biblical narrative but had never really been in opposition to it, in spite of the many historical problems involved.

So, Liverani published his own version of a history of Israel, a history that was divided into two sections—as is reflected by the not-too-precise English title. The first part is a “standard” history, a reconstruction of the history of the landscape of Palestine from the Late Bronze Age to the time of the Jewish Diaspora; the second is about the intentions of history writing in the Old Testament, “an invented history.” The first part is more traditional than was to be expected from this author. When we get to the two kingdoms of Israel and Judah, he leans primarily on biblical historiography. In many ways he gets no further than, say, Max Miller (or for that matter Gösta Ahlström in his magnum opus on Palestinian history from 1993).

The second part is really interesting because Liverani here, in a way not so different from the strategies followed by Thomas Thompson, centres on the meaning of texts: Why were they written? What purpose did they have? Here I can follow Liverani in almost every respect. Thus when he titles his chapter about the Judges “A Nation without a King: The Invention of the Judges,” he is absolutely right. The separation of the final parts of the book of Judges from the preceding parts is disastrous for the ideological coherence of the book. The motto at the end of the book is “In those days there was no king in Israel; all the people did what was right in their own eyes” (Judg 21:25), meaning that the theme of the book might therefore be that the introduction of dictators—“judges”—was no success. This is illustrated among other things by the chronology presented by the book: the periods of strife become longer and longer, and the periods of peace concomitantly shorter and shorter. So, while it started well with the sly Ehud (although his treatment of Eglon [Judg 3:22] is not a nice way to kill a man), and it proceeded well under Deborah and Barak, by the time of Gideon we begin to see that this institution is becoming disastrous. Despite turning down the offer to become a king, Gideon is behaving like one, and his son lives up to the role by usurping kingship and killing his family. A successor to Gideon, Jephthah, the product of Gilead’s liaison with a whore, sacrificed his daughter like another Agamemnon—a character not deemed mildly in Greek tradition—and ultimately got his people engaged in a bloody civil war. Finally, the last judge was the brainless bow-wow hero Samson. Clearly, this is not historiography in any modern sense; it is a reconstructed historical tale from the hoary past.

Liverani shows that we do not move from history to ideology but from ideology to history—if there is any—which Liverani believes. This is exactly what Thomas Thompson, across hundreds of pages, has tried to explain, though, it seems, without really being understood by those biblical scholars who still believed in historicity. Without such an understanding there is no hope of ever writing a history of Israel that is not just another biblical hypertext. There are too many questions not addressed, and too many problems not solved, and these have to do with hermeneutics, not history as such. If we do not bother, I guess that few will pay attention to what we are doing. It is also possible to write a history of Palestine “oltre la Bibbia,” without the Bible. I will shortly present my bid for such a history.⁷

7. In Lemche 2008, Appendix: “The History of Israel or the History of Palestine?”

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A “NEW CULTURAL HISTORY” OF EARLY ISRAEL

Robert D. Miller II

Introduction

In recent years, Norman Gottwald, Megan Bishop Moore, Diane Banks, and I have argued in several venues for methodological perspicuity in writing the history of ancient Israel (Gottwald 2004; Moore 2005; Banks 2006; Miller 2003b, 2006).¹ Noting that a large percentage of what is written in our field—by minimalists and maximalists alike—has not moved beyond the positivism of von Ranke (Miller 2003b; 2006, 152–54; Moore 2005, 100–104),² we encouraged scholars to look to developments in historical theory, especially postmodern social history (Burke 1997, 77, 79, 93, 165). Social, because rather than study events, scholars would study the structural context of all social acts, systems that give meaning to society (Hodder 1982b, 187; Gottwald 2004). Postmodern, because the result would be a history that does not “nail things down...” but “pries them loose” (Darnton 1986, 231). We would write histories that are well-argued plausibilities, possible pasts, which are available to further testing and examination and which challenge other possible pasts, yielding better-informed reconstructions (Miller 2003b; 2006, 157–59; Gottwald 2004).

There were already several studies that tackled the sort of social history we proposed. And some explicitly espoused the idea of “possible pasts.” Among the earliest, studies included in Philip Davies’s 1991 edited volume, *Second Temple Studies*, had proposed possible pasts for postexilic Judea (Davies, ed., 1991).³ D. W. Jamieson-Drake had

1. Similar concerns were raised 20 years ago by Frederic Brandfon (1987, 5–43). Throughout this essay, I freely attach the label “Israel” to the Iron I highlands of Palestine, for reasons presented thoroughly in Miller 2004, 55–68; and reiterated in Kletter 2005, 581–82. For dissenting views, see Faust 2005, 494, 496.

2. For modern Rankeanism, see Lemche 2005, 17, 19, 22, 25; but cf. 21–22.

3. For example, Hoglund (1991) reconstructs the economy of Judea from archaeological data and Persian documents and then makes second-level deductions about the sociology of Judea based on this assumed economy.

presented reconstructions for monarchic Judah, using anthropological and geographic models to interpret archaeological data (Jamieson-Drake 1991). More recently, for the settlement period, Uta Zwingenberger's (2001) reconstruction of village life in Iron I Palestine drew on ethnographic analogy to present a model for community life. Each of these studies proposed plausible, admittedly non-Rankean, reconstructions of a non-objective past. Zwingenberger, Jamieson-Drake, Avi Ofer (1993, Fig. 94), and I (Miller 2004b, 57–63) utilized anthropological models to explore the finite instances of observable patterns that one can isolate in the archaeological record and historical context (Geertz 1973, 10; Hodder 1989, 250; 1992, 162, 170; Morris 1992, 6; Lamberg-Karlovsky 1993, 288; Shanks 1997, 398; Trigger 1998, 29–30). Other recent examples of (at least somewhat) postmodern, systemic, social history-writing include Robert Coote's 2005 "Tribalism" essay, Carol Meyers's 2006 "Hierarchy or Heterarchy," Mark Sneed's *Concepts of Class in Ancient Israel* (1999), and, on the largest scale, Gottwald's *Politics of Ancient Israel* (2001) and Mario Liverani's *Israel's History and the History of Israel* (2005).⁴

Yet, in the field of historical theory, historians have rejected the nomothetic comparative method and its quest for universal laws. This is because the atemporal forms, phyletic homologies, types, formulae, laws, or anthropological "structures" that the comparative method assumes exist in multiple settings (Davis 1990, 28; see Gottwald 2004 for examples) simply cannot exist except on a purely morphological level—societies are simply not pushed out of a mould that renders only three or four distinct "types" of society. Critics (e.g. Lemche 1996, 284) are correct that anthropological models like the complex chiefdom I proposed (2004a) are structuralist (the society may be seen as merely the "types" just alluded to), processual (every event or individual is merely a functional contingency of the society's process), reductionist (eliminating the idiosyncrasies of the society that may not perfectly fit the model), positivistic, and mathematically deterministic, and that their goals are to write an equally structural (or processual, etc.) social history, interested only in the society as a demographic organism (Shanks and Tilley 1987, 44, 52; Morris 1992, 5; Renfrew 1994a, 47; Banks 2006, 174–84). My chiefdom model focused on pigeonholing societies into systems and persons into role theory, and obscured the individual and ideas (see Morris 2000, 17; 1992, 22; Megill 2004, 221; Hodder and Hutson 2003, 217;

4. We could also add Snell (1997, 135), were it not for its highly uncritical use of the Bible.

Lemche 1996, 280; Paynter and McGuire 1991, 10–31).⁵ "Ideas" were seen as embedded in long-term social process and relegated to the *social* "history of ideas" and the formalist-idealistic assumptions it involves" (Henning 1982, 154; Davis 1990, 28). Ignored are the "relatively few, highly complex and ambiguity-ridden concepts around which the social organization of a culture revolves and the emotional and intellectual energy of its members is largely spent" (Polanyi 1981, 99). Only such concepts move history beyond the social history of economics and politics into cultural history (e.g. Moore 2005, 106; Hodder and Hutson 2003, 129), attempting to reintroduce the individual and to look at the role of ideas in social process (Hodder 1987, 144; Dever 1993, 708; 1997, 179).⁶

New Cultural History was the title of a 1989 volume of essays edited by Lynn Hunt that heralded the advent of the project, an approach that emerged out of the meeting of poststructuralism and the anthropology of Clifford Geertz (Megill 2004, 222; McMillan 1994, 759).⁷ The new cultural history holds that social categories, "vary from place to place and from epoch to epoch, sometimes from year to year. As a result, the quantitative methods that depended on social categories fell into disrepute almost as soon as they came into fairly widespread usage" (Bonnell and Hunt 1999a, 7; cf. Morris 2000, 11). Just as some of us were discovering social history in biblical studies, "The social began to lose its automatic explanatory power" (Bonnell and Hunt 1999a, 8).⁸ Indeed, a plea for the absolute necessity of culture in writing Israelite history was raised by Ferdinand Deist in 2000, and William Dever has called for a move beyond processual archaeology's scientism to "reading material culture" (Deist 2000, 47–48; Dever 2004, 517).

Cultural history depends "on the interpretation of meaning rather than a scientific discovery of social explanations" (Bonnell and Hunt

5. For defense of "process" against the renewed emphasis on the individual, see Flannery 1999, 17–18.

6. Some of the demise of systemic, social history came from the failure of Marxism as an interpretive paradigm in the face of the collapse of Communism; "the failure of Marxism has signaled a more general failure of all paradigms" (Bonnell and Hunt 1999, 4; also Cross 1998, 245). Another reason for the demise was that "the more that has been learned, the more difficult it has become to integrate that knowledge into existing categories and theories" (Bonnell and Hunt 1999, 10).

7. Within history itself, some have already derided New Cultural History as "currently the hegemonizing and imperializing fraction of the discipline." I do not propose New Cultural History as the only legitimate approach, but it is one that addresses several shortcomings of social methods (Megill 2004, 223, 226).

8. For examples of earlier efforts to subsume culture under social processes, see Hodder 1982.

1999a, 9; Hodder 1987, 144). This does not mean giving up explanation as a goal of history writing (contra Haag 1995, 202). Cultural historians “have not given up on social or causal explanation; rather, they seek better explanations” (Bonnell and Hunt 1999a, 25). For example, even when actions in ancient society are almost entirely determined by economics or political structure, those socioeconomic patterns would still have to be rendered meaningful in action for ancient people in language or in some other form of symbols (Sherratt 1996, 155–56; Davis 1990, 29; cf. Renfrew 1994a, 47; 1994b, 5).⁹

There is considerable debate about the meaning of “culture” in “cultural history.” Even if, following Leslie White and Lewis Binford, it is “man’s extra-somatic means of adaptation” (Renfrew 1994, 3), “Is it an aspect of life, like society or politics, or a way of defining a certain set of beliefs and practices?” (Bonnell and Hunt 1999a, 12). Some scholars “emphasized *practice* in order to oppose what they see as an overly *linguistic* of discursive definition of culture” (Bonnell and Hunt 1999a, 12; italics added) in phrases like Peter Burke’s “Poetics...which underlie everyday life in different places and times” (Burke 2001, xxiii). “Much of the theoretical writing on culture during the past ten years has assumed that a concept of culture as a *system of symbols and meanings* is at odds with a concept of culture as *practice*” (Sewell 1999, 46; italics added; Scribner 1989, 180–81). “System and practice are complementary concepts: each presupposes the other. To engage in cultural practice means to utilize existing cultural symbols to accomplish some end” (Sewell 1999, 47).¹⁰ But at the same time, cultural history (exemplified in this respect by Clifford Geertz) recognizes that actors in society can play on the multiple meaning of symbols, redefining situations in ways that they believe will favor their purposes (Sewell 1999, 51; Silverman 1990, 126). Culture is “in continual flux...a pool of diverse resources, in which traffic passes between the literate and the oral, the superordinate and the subordinate, the village and the [comparative] metropolis; it is an arena of conflictual elements, which requires some compelling pressure...to take form as ‘system’” (Thompson 1993, 6). We were simply mistaken that a culture’s most important beliefs are consensual, agreed on by most of a society’s members (Sewell 1999, 54; Burke 1990, 185; cf. assumptions made by Ahlström 1984, 118). There were surely examples of resistance to the well-oiled complex chiefdoms of early Israel. Yet,

9. Contrast also the sentiment of Daniel Snell: “get behind political propaganda to an economic and social reality” (1997, x). Likewise, Gottwald (2004) holds that “a society’s ideology itself forms one of its networks of public power.”

10. “Culture [even] creates differences that are not real” (Anfinset 2003, 56).

“we can achieve an adequate understanding of material culture *without* attempting to reconstruct the specific intentions of individual agents” (Tilley 1990a, 339; italics original; Haag 1995, 203).¹¹ We do not have to explain some notion of systems by *either* a social totality like a chiefdom model *or* an irretrievable mind of the individual, if all social relations and material culture are *not*, in fact, linked in some continuous manner (Tilley 1990a, 339).

The biblical text might provide information about such culture for early Israel; it would be most difficult to prove that it does not. There is value in the “interpretive communities” of the ancient Israelites themselves, who “represented material culture as something to use creatively, in the same way as words, to construct images of themselves and the world around them” (Morris 2000, 25). Ian Morris illustrates how central Greeks of the 8th century started writing heroic and theogonic literature and, in so doing, revolutionized their relationships with the past “in the face of traumatic shifts in popular consciousness at a time when everything they believed in was under threat but which also offered them unimagined new possibilities” (2000, 261; cf. 267).

However, such information appears inextricably bound with thick cultural concepts, intentions, and concerns of the author’s own period and of many others before his time. The biblical text cannot be used as a direct source for reconstructing cultural concepts (Deist 2000, 106–7; contra Flanagan 1988, 114). Given the near impossibility of identifying texts of a specific timeframe, we may be reduced to Ferdinand Deist’s generalizations about Hebrew language itself: it tends to classify abstracts, natural forces, and limited areas as feminine, actors and wholes as masculine.¹²

Archaeology, in its study of material culture, on the other hand, is “one of the arenas in which culture and social life most obviously and significantly intersect” (Bonnell and Hunt 1999a, 11). All artifacts have a symbolic, ideological side—distinct according to context in most cases—and ideology does not exist independently of material culture (Bard 1992, 3–4; Morris 2000, 26; Zubrow 1994a, 189). Moreover,

11. In fact, “The ‘individual’ is a very recent construct, tied closely to the development of modernity in the West” (Hodder and Hutson 2003, 7; Meyers 1997, 39). Frank Cross is right to question any suggestion of the “novel” or “innovative” in the development of culture (1998, 241; Burke 200, 60, 84, for more cautious views). Faust (2006, 32–33) exaggerates the role of individual agency in order to prove the existence of King Saul.

12. This should not reduce us to the pure fantasy of van der Toorn (2004, 393–410).

archaeology is suited to “reading” the material culture as text, for understanding that ideological side of artifacts (Davis 1990, 29). The “heavy symbolic, semiotic loading of artifacts and the artifactual process helps one to understand the larger [culture]” (Roe 1995, 59). Thus, Ian Morris writes, “Archaeology is cultural history or it is nothing” (2000, 3).

One avenue might focus on measurement of artifacts. Specific, designated weights, capacities, and dimensions are a symbolic reality that has been imposed on material culture for different practices (Renfrew 1994, 7). In fact, all *topology* (not typology) of artifacts, the properties independent of size or shape that the potter brings to bear on his or her dealing with shapes, is fundamental to the conceptualizations involved in manufacturing practice (van der Leeuw 1994, 136). Cultural conceptualization need not be altogether conscious (Weissner 1984, 193): “mere” adherence to a cultural norm because it is “right” is the message of identification with that culture (Kamp and Yoffee 1980, 96; Weissner 1984, 193; Burke 2001, xxiii). Even if the style is isochrestic, “iconography of the commonplace” by common sense rather than design (Sackett 1977, 377–78; 1985, 157), that does not mean that it is not signaling. Such “common sense” in the way artifacts are manufactured “is as much an interpretation of the immediacies of experience, a gloss on them, as are myth, painting, epistemology, or whatever...it is, like them, historically constructed” (Geertz 1983, 76; cf. Burke 1990, 189). There is a spectrum between blatant explicit symbolic style and isochrestic variation or cultural norm. Actually, there are hundreds of social and contextual factors that affect style and its interpretation—ranging from sexual politics to linear or cyclical time-view of the culture, added to dozens of individual level factors (listed exhaustively in Roe 1995, 40, 42–43; also Braun 1995, 132). Styles are *not* conservative, but allow considerable invention (Roe 1995, 45–46).

Nevertheless, some general rules apply (Voss and Young 1995, 93–94; Pryor and Carr 1995, 275). The spatial extent of a culture is reflected by the distribution of highly visible stylistic attributes with a discrete distribution (statistically taking on only discrete values, its probability distribution increases only where it “jumps” to a higher value). Interaction intensity between groups and a lack of sharing specific meanings will be reflected by high-visibility attributes with distribution of frequencies that show a systematic gradation over space (“clinal” distribution, whose probability distribution can be expressed in terms of integrals or is gradual over a region). Isochrestic variation tends to display with low-visibility attributes of discrete distribution. Forms of interaction below

the scale of shared culture¹³ and across cultural boundaries blur what distinctions are produced at these levels by shared cultural histories (Pryor and Carr 1995, 291–92). Rapid change in style is usually triggered by external influences and cultural instability (Rosenthal 1995, 362).¹⁴

Additionally, “Space is practice (our everyday actions); it is also symbol” and a tool for memory, and so domestic architecture can be particularly important (Pearson and Richards 1999, 5; Fabian 2002, 111). “The organization of space...transmit[s] messages at one level about the men and women who live in it and at another level about the culture in which they live” (Burke 2001, 81). “Focus” and orientation in village plans is an extension of such reading (Pearson and Richards 1999, 12, 14). “The structuring of space incorporates cosmological and symbolic principles in many situations” (Pearson and Richards 1999, 38). Practically, this is because “inhabited architecture facilitates the orientation of the body’s movement, it directs progress from one place to another, it enables activities to be assigned to particular places, it orientates and focuses the attention of the practitioners” (Barrett 1999, 91). Writ larger, this involves regional settlement patterns, seen now not as archaeological correlates of particular social systems but “as a means of situating the subject’s control, or the control of the community to which that subject belongs, over resources [that] can be considered as operating either on *places, paths, or over areas of ground surface*” (Barrett 1999, 93; *italics original*; Nash 1997, 57). Space is “categorically defined” by human actors (Zubrow 1994b, 114). As an example, Ian Morris illustrates how central Greeks of the 8th century “redrew the boundaries around their communities” (2000, 257). Put another way, all “settlement archaeology” are investigations of “ritual landscapes” (Sherratt 1996, 149).

Quite clearly, religion, broadly defined, is a crucial element of cultural history.¹⁵ The archaeology of religion will include both architecture of special places and the artifacts associated with such places (Renfrew 1994a, 49). Significant also will be mortuary information, indicative of ideology much like contemporary texts (O’Shea 1984, 18; Bard 1992, 3). Since Iron Age tombs are so rare in Palestine, it is helpful that this post-processual archaeology does not rely on statistical analyses of mortuary data (Hallotte 2001, 190).

13. I hesitate to call this scale “ethnicity” for reasons explained in Miller (2004b, 55–56; cf. Anfinset 2003, 50).

14. Even cultural anthropologists will revert to a social causation when explaining such change (Wagner 1981, 105).

15. Renfrew warns, however, that religion ought not to be reduced to a social phenomenon without reckoning with religious experience itself (1994a, 48).

Ekistics

Ekistics is the science dealing with human settlements and drawing on the research and experience of architecture, settlement geography, and sociology. It subsumes several of the areas of importance for a cultural history of early Israel. In all of these, the premise is that, “The relationship between things is inscribed in space: it is the space between things... that creates the relationships which we experience. It is from these experiences that we interpret meaning...in the archaeological record” (Hitchcock 1996–97, 47).

We may begin with house design, orientation, and access analysis, since, “A house is not just a machine for living in but also a centre of ritual... Houses are bound to reflect, and to shape, family life and habits of work or leisure; they too may be seen as systems of signs” (Burke 2001, 81; Robin 2002, 247). The so-called “Israelite Four-roomed House,” which is not at all unique to Iron I Israel,¹⁶ is a quite standardized form in the Iron I highlands. Its ubiquity is attested in examples at Dothan (Area A), Izbet Sartah (House 109b=112b, House 310), Ai (South Bench House in Area B, House 189), and Khirbet Raddanah (Site S).¹⁷ The regularity of the form may reveal an ideology of order and unity (Bunimovitz and Faust 2004, 419). The house form, which allows each room to be entered directly from the main room, can be contrasted with most Late Bronze Age and lowland Iron I houses where each room is entered from the previous one. Bunimovitz and Faust interpret this as allowing ritual purity to be maintained by isolating the impure in a given room (2004, 419), although the nature of isolation could just as easily be based on gender as on (biblical) purity laws.

There are problems with this analysis of the four-roomed floor plan. Houses would have been made of mud bricks, with a stone foundation and perhaps a second story of wood—tamarisk or poplar or palm. It was possible to build a stone house; there was plenty of stone. However, the area was earthquake-prone, and so it was dangerous to construct out of fieldstones. Above the top story could have been a sleeping roof or a

16. There are LB examples from the Euphrates and Amuq plain, and Philistine cases from Iron I at Gezer (House 167) and Tel Qasile (House Stratum X), and antecedents go back to the Neolithic (Nur ed-Din 2000, 506–7).

17. All archaeological data are taken from Miller (2003a), supplemented by more recent publications (Livingston 2003, esp. 47–50, 54–64; Master et al. 2005, esp. 67–86; Van Der Steen 2004, 148–51; Zertal 2004 is an English translation of material used in Hebrew during the writing of Miller 2003a and provides no new information), unless otherwise noted.

covered roof loft. “It now seems clear that the ground-level space in the pillared dwellings represents a specialized layout that met the needs of agriculturalists” (Meyers 1997, 15). Thus the large, first floor room was a court for the animals, similar to the Arabic *Qa'* or *Rawieh* (Canaan 1933, 35; Holladay 1997, 3:96), whether it was covered or not (Nur ed-Din 2000, 507). Bunimovitz and Faust’s analysis depends on *all* the ground floor rooms being for human occupation.¹⁸ This central room may have been partially open, although most food processing and textile activities would have required space outside the buildings, where most cooking ovens and hearths have been found (Meyers 1997, 16).¹⁹ Although the Israelites lived in nuclear families, there were clusters of houses around common courtyards (as the Arabic *Hosh*).²⁰ The relation of this arrangement to ideology and gender will be discussed below.²¹

Town design is an extension of this analysis. Unfortunately, Iron I “villages were generally laid out rather haphazardly” (Meyers 1997, 12; for Iron II, on the other hand, see Faust 2003, 123–38). Nevertheless, generalizations about the functions of an Iron I highland farming village will influence town layout (Miller 2003c, 289–310).²² This much is a given tenet of social archaeology. But it is also true that a social reconstruction of everyday life may give the best purview on culture, as well. Bob Scribner has argued that, rather than focus on such things as oral tradition or even “cultural” artifacts, we ought to model daily life and then understand its cultural implications (1989, 182; also Burke 1990, 187).

Taking this approach, village tasks in the highland calendric economy, based on reconstructed economic strategies and ethnographic analogy (Miller 2004a, 63), can here be divided according to gender. In the winter, men plough for legumes, handle the second plowing, and plough for green fallow. Men thresh the grain in the summer. Additional male tasks would have included house and cistern building and repair. While

18. Also problematic is that Bunimovitz and Faust “largely present a perspective that is functionalist and somewhat static,” paying scant attention to ways in which the “manipulation of space” can “generate” meaning (Hitchcock 1996–97, 49; *italics added*).

19. This is also the case with the nearly identical New World house known as “Type 1” of the Antelope Creek phase in the Texas panhandle (Lintz 1986, 152).

20. This is also the case with the New World house (Lintz 1986, 102).

21. All definitions of “house-cluster,” “courtyard,” “neighborhood,” and even “domestic space,” are culturally conditioned, and we should be wary of defining any of these from our outside perspective (Robin 2002, 247).

22. More recent evidence confirming the former’s conclusions includes Livingston (2003, 64).

both men and women sow cereals, lentils, chickpeas, and other legumes, as well as dig the vines and olives and participate in the harvest, the only agricultural role away from the village that was solely women's was the spring weeding.

Roles more likely to take place at a distance from the residential compound were thus primarily for males. Where the Israelite communities relied in part on domesticated animals (and faunal remains from Shiloh, Ai, and other sites show the importance of grazing in the economy), these were grazed on distant land up to 15 km away, usually by boys (Grant 1921, 142). "Judging from the osteological data, hunting played a secondary, but surely not insignificant role in the economic system of the Iron Age I highlands peasant" (Rosen 1994, 340), and this is a male job. Culturally, the inclusion of grazing (and at such a distance) and hunting "trouble[s] the division between the infield as fenced in and permanently tilled, and the outfield as an area that has not been cultivated," if not calling into question entirely such a "dualist division" (Holm 2002, 70). The men and boys involved in these tasks "must also have had ways of becoming more familiar with the landscape and taking it into use" (Holm 2002, 75). Ethnographic analogies suggest such practices as assigning place-names, composing songs about the landscape, and otherwise "transforming an unknown landscape to a cultural landscape with its own history" (Holm 2002, 75, with cases from Iron Age Norway; Basso 1996, 61, 81, with cases from the Apache).²³ They suggest "a worldview with shamanistic elements...[that] emphasizes the landscape as unlimited space and nature as controllable and not dangerous" (Holm 2002, 77).

Women's productive tasks "involved keeping the home in order, caring for small children, tending gardens and small animals, producing textiles, and taking responsibility for food preparation and preservation" (Meyers 1997, 25). These tasks, especially the last two, were both very time-consuming and involved highly specialized technology and skills (Meyers 1997, 25). This productive work would have had to proceed largely unaffected by the significant proportion of women's lifespan taken up with childbearing (Meyers 1997, 28). On the level of culture, or ideology, therefore, biblical androcentrism should not so mask female agency as to merit the label "patriarchal" (Meyers 1997, 34; Nunn 2006, 72). "Patrilineal," "patrilocal," and even "androcentric" may be accurate terms—the former two are purely social and the latter only a postulate from the biblical data—but a cultural history of early Israel cannot support 19th-century Western gender images.

23. On the relation of place-names to cognitive categories, see Basso 1996, 44. Such toponyms approximate Mikhail Bakhtin's "chronotropes"; see Bakhtin 1981, 7.

On the largest scale, settlement patterns are also a form of ekistics. We may first note that although there appear to have been many more Late Bronze Age highland sites than earlier maintained, there are still a great number of newly founded sites in the Iron I period, sites founded on no previous Late Bronze Age occupation (see the summary data in Van Der Steen 2004, 101). Culturally, these new settlements and new settlement patterns indicate new, different reactions to the meaning imposed on the landscape by its Late Bronze Age inhabitants (Sørensen 1992, 133, with archaeological examples from the Als region in Denmark).

Elsewhere, the settlement map of the central Iron I highlands has been divided up into economic "bailiwick" by Theissen polygons, Dirichlet regions, and Voronoi cells (respectively, Miller 2004a, 36–41; 2004b, 57–63; Forthcoming). Overlaying these with environmental regions, each bailiwick spanning several, pointed to the complexity of the highland economies (Miller 2003c, 291–95). One cannot even pull a given site out of one of the environmental regions and read that as its subsistence strategy. Within each region, there are variations dependent upon rainfall, altitude, position on the hillside (both directional and elevational), requiring each site individually to be identified according to these features. Clearly, even where all factors are equal, there is often a choice of strategies, even a choice between such diverse strategies as cereal farming, grazing, and horticulture (and hunting, fishing, etc.). Given the small-scale specificity of subsistent strategies and the diverse technical knowledge required, early Israelite culture would have "required the exercise of considerable parental guidance... Parental interaction with children, insofar as it was organized around bringing the children into the orbit of household productivity, required a considerable level of authority" (Meyers 1997, 31). There were probably "extreme penalties attached to legal strictures that aimed at ensuring parental authority" (Meyers 1997, 31), and these, like all difficult behaviors necessary for the overall survival of the group, would have been presented as divine mandates (Meyers 1997, 29). But perhaps simultaneously, "household religious or ritual activities, along with family storytelling traditions, served to preserve and pass on unifying elements of connected family households" (Meyers 1997, 31). As Natalie Zemon Davis notes, "More is packed into the meaning of ritual life than we have thought" (1982, 326).

These bailiwick describe territories of a social nature—economic and possibly political. But settlement patterns are also manipulated, redrawn, and categorically defined. This can be seen by applying a "rubber sheeting" algorithm to ideal and real settlement patterns (Zubrow 1994b, 113). The biblical text postulates that its tribal allotments are accurate depictions of Iron I phenomena, and, to an extent, this postulate can be

tested. The method requires comparing the biblical tribal map with the archaeological settlement map (Shimizu and Fuse 2005, 1–3; Monti, Guerra, Balletti, and Miniutti 1999).²⁴ Should the rubber sheeting show a high degree of correspondence between the tribal boundaries and the polygon boundaries, this might indicate that economic factors alone were not responsible for the settlement pattern, but that ideological motives also contributed to the founding of sites in the places they were. A lack of correspondence might indicate that *between the time* of the early Israelite settlement and the codification of the tribal boundaries—which could be quite late—the map was redrawn for “cultural” reasons.

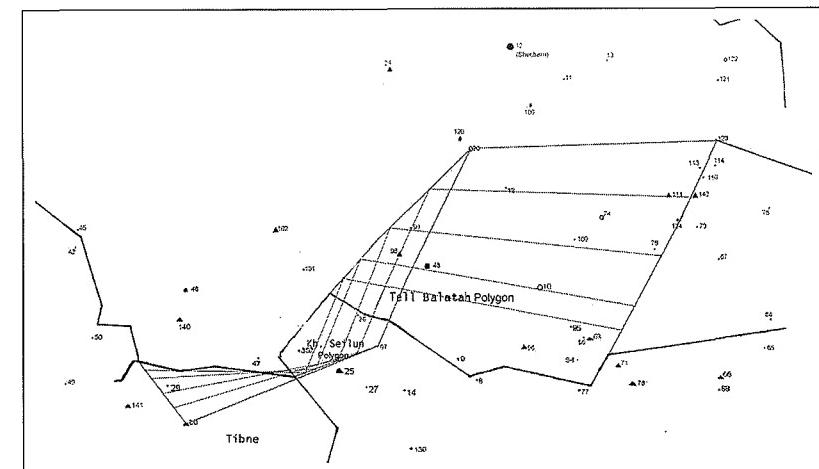
The rubber sheeting method is straightforward. It requires, however, a certain degree of rough congruity between the two sets of polygons (Doytsher 2000, 1005; Saalfeld 1985, 169). There is only minimal congruity between the archaeologically derived polygons and the tribal regions of the Bible. Thus, there is no biblical boundary between the region of Ephraim around Shiloh and the region further west around Timnath-heres, in spite of the presence of two polygons, one around Shiloh and one around Khirbet Tibne (Miller 2004a, 38 Fig. 6). There is no biblical division of the zone of Tirzah and the zone of Dothan from the Manassite region around Shechem, although Tell el-Farah North and Dothan are clearly distinct polygons from Tell Balatah (Miller 2004a, 38–39). For these reasons, it is impossible to use rubber sheeting to compare the polygons; they are too dissimilar.

The biblical border between Ephraim and Benjamin is far to the south of the archaeologically derived boundary between the territory of Shiloh and “Benjamin” (Miller 2004a, 38 Fig. 6). The biblical boundary runs south of Ai, Bethel, and Beth Horon, while the polygon boundary runs further north, between Khirbet Mneitra and Khirbet Tarafein. The only possible correlation is that the ancient polygon of Shiloh extends far to the south along its eastern side, cutting “Benjamin” off from the Jordan River (Miller 2004a, 38 Fig. 6, 89 Fig. 26), just as the biblical boundary of Manasseh (not Ephraim) extends far to the south along its eastern side, cutting Ephraim off from the Jordan River.

The biblical border between Ephraim and Manasseh likewise does not match the boundary between the Shiloh polygon and that of Shechem (Miller 2004, 38 Fig. 7). East of Sheikh Abu Zarad (biblical Tappuah),

24. Mountains, rivers, and other landscape features that are immobile form control points on both maps. Then, using Delaunay triangulation, a TIN (triangulated irregular network) is formed over all those control points, ensuring that they are homeomorphic. A planar affine transformation is then made for each triangle pair, or the fifth-order polynomial transformation is employed with conditions such as continuity and smoothness of adjacent piecewise transformations.

the biblical boundary is too far to the north, swinging north of Khirbet el-Urmah (biblical Arumah), while to the west of Sheikh Abu Zarad, the boundary is too far south, passing south of Khirbet Matwi. Nevertheless, along this region it is possible to employ a simple form of rubber sheeting (White and Griffin 1985, 124). This is illustrated in Fig. 1. It shows some vague congruity between the borders, but also adds to the conclusion that the biblical tribal boundaries do not reflect realities from the Iron I period but more probably come from later Israelites who “redrew the boundaries around their communities” (Morris 2000, 257).



“Ephraim–Manassite” erect/slanted-rim cookpot and the “Benjaminite” plain- or ridged-rim ones (Bunimovitz and Finkelstein 1993, 157). There is even not much of a problem subdividing the first category into Late Bronze-ish cookers, triangular-rimmed “Israelite Pots,” and non-carinated varieties (Zertal 1994, 5). The problem is that at this level, or at the previous one, the results for the entire highlands are so skewed in favor of the triangular-rimmed “Ephraim–Manassite” pots as to make the other types insignificant.²⁶ Some scholars do subdivide the “Israelite Pot” further (Finkelstein 1988, 271), and this would give a good distribution. But there is debate about whether these small subdivisions are chronological (Zertal 1994, 53), regional (Finkelstein 1988, 271), or potter-specific (Dever 1991, 84). So, cooking pots will not be used.

This leaves store-jars, for which there is a good distribution of varieties for plotting, although the collared-rim pithos will not be subdivided as it could be (e.g. Finkelstein 1988, 276; Master et al. 2005, 77), as there are not enough data distinguishing occurrences of its subtypes.

Yet, this vessel’s typology is not a clear example of a “highly visible stylistic attribute” (Pryor and Carr 1995, 291–92). Perhaps more useful would be the occurrence of puncturing decoration of jars and jugs (Zertal 1994, 54; Finkelstein 1988, 279). It is not clear whether it is worthwhile to distinguish between simple puncturing and the “man’s face” punctate design, or between puncturing on handles and puncturing on rims, and so the plot could simply be of the presence of puncturing. Again using the data assembled in 2003 (Miller 2003a, 143–218), however, it seems puncturing occurs just about anywhere: at sites of all sizes, of all levels in the systems, and in all of the systems. There does not seem to be any cultural significance to the occurrence of punctate design. The only interesting points are that puncturing occurs in only one site of the Khirbet Tibne system (Deir el-Mir, a B-level center), and only one site in the Tell Dothan system (Joret el-Ward, a B-level center).

So we must return to store-jar types, the occurrence of which may be meaningful for the regional distribution of the type, perhaps dictated by culture. The distribution is *not* socio-economic: the jar types occur at random at any level of site and at any size of site (Miller 2004a, 41–50; Boraas 1999, 21). But they vary by region (Fig. 2). South of the Tell Balatah system, only the collared-rim jar (hereafter Type 1) and the hole-mouthed jar, having a rim with sharply marked edge along outer circumference (hereafter Type 8), occur. In the Tell Balatah system,

26. There are 84 Ephraim–Manassite to three Benjaminite; if we divide the former, 66 “Israelite pots” to 18 LB-ish pots, four non-carinated, and three Benjaminite.

these two are supplemented by the occurrence of simple-rim, low-necked jars (“Type 7”) at three sites and ridge-necked (on neck below thickened rim; “Type 3”) and simple-rim, high-necked (“Type 6”) at one site (Sebastiyya). The Tell el-Farah system has the most variety, with Type 3 at two sites, jars with a ridge at the bottom of the rim that has merged with the rim itself, not separate on the neck (“Type 5”), at one site, Type 6 at two sites, and Type 7 at one site, besides Types 1 and 8.²⁷ In the Tell Dothan system, Type 8 store-jars are no longer found, and Type 1 is found alongside Types 3, 6, and 7.

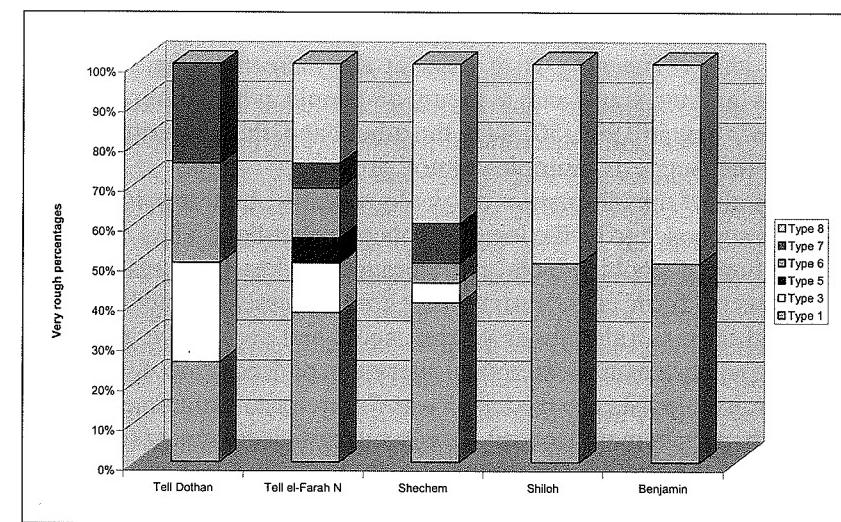


Figure 2. Distribution of Pithoi Types

Since what are changing are the tops of storage jars, these variations are not highly visible attributes. There is great debate over whether pithoi are too heavy to transport, which is related to their visibility. Michael Artzy argues that camels could easily have carried them, as depicted on Egyptian art, and that evidence from Tell Jemmeh, Cyprus, and Egyptian New Kingdom art shows that they were domesticated in the mid-second millennium B.C.E., the Late Bronze Age (Artzy 1994, 134–45, 137). Most scholars doubt such an early domestication, and in any case, there are no abundant camel remains until the Hellenistic period (Hesse 1995, 1:217). However, one could always transport the pithoi by oxcart, as depicted on Egyptian paintings (Wengrow 1996, 308; contra Killebrew

27. “Einun jars” have been removed from this calculation, given the possible validity of Finkelstein’s re-assignment of these to MB II–III (1998, 94; cf. Mazar 1999, 146).

2005, 270). So, it is not certain that pithos designs are “low-visibility” variations, either. The distribution is more clinal than discrete. The finds do suggest that the bailiwicks do not represent distinct cultures and that there is no evidence for interaction intensity between groups and a lack of sharing specific meanings (Pryor and Carr 1995, 291–92).

This does not mean that all of these polities were “Israelite” or that they were part of a pan-tribal league, amphictyony or otherwise (Lemche 1985, 301). This evidence “does not mean that they form a community in any fuller sense... The semiotic field they share may be recognized and used by groups locked in fierce enmity rather than bound by solidarity, or by people who feel relative indifference toward each other... It simply requires that if meaning is to exist at all, there must be systematic relations among signs and a group of people who recognize those relations” (Sewell 1999, 50).²⁸ On the other hand, there is nothing in the plotting that suggests multiple cultural groups.

But is all of this discussion of typology really about topology at all, which is supposed to be the locus of conceptualization? The typological distribution of cooking pots and storage jars has been restricted to variations in “form” and “variety,” the kind of vessel (defined functionally) and terminal alterations of rim, neck, and so on. The punctuate design is solely a matter of terminal alteration, of decoration. Syro-Palestinian archaeologists use the term “ware” to refer to technique (process of construction in manufacture), fabric, and surface treatment (outside of the Levant, the latter is not included in “ware”), and *these*, and not the rims, would seem to be the properties of the pottery that are independent of size or shape, the topological distinctions. Ought we, on a grander scale, to question the entire typological method of Syro-Palestinian archaeology, its crude and clumsy “Q-Analysis,” or tabletop associations between similar objects? Certainly, an increasing number of Mesopotamian and Egyptian archaeologists, and most anthropologists in the world, argue that statistical “R-Analysis” is preferable, that quantifiable factors should be used to classify objects.²⁹ It has been argued since the 1950s that classification into types ought *not* be based on “a site-to-site comparison designed to show consistency of the identifying pattern, range of variation [e.g. collared-rimmed store jars], and historical relevance” (Spaulding 1953, 305; also Binford 1965, 203–10). Rather, classification should be “a process of discovery of combinations of attributes favored

by the makers of artifacts,” not arbitrary attributes chosen by the archaeologist (Spaulding 1953, 305; *italics added*). To this point, no one seems to ask if the ancient Israelites would have categorized their vessels as we do. Rough R-Analysis is not difficult, using, for example, Quantitative or Qualitative Cell Tables made by lining up rows of one type of attribute (e.g. temper) with columns of another attribute (e.g. surface finish). The categories for the typology are then determined by combinations of the rows and columns, although a rather complex mathematical procedure is then involved (Spaulding 1953, 306–12; with emendation by Cowgill 1982, 40, 45, 47). Simple, but not of interest to Syro-Palestinian archaeologists.³⁰

Yet, in the end, even topology may not be a good avenue to culture; perhaps the ancient potters did not want their “message” to last as long as a pot surely would, and so would intentionally not “encode” a style-message in pottery. That is, there may have been a sort of durability hierarchy at work that is irrecoverable (Pollock 1983, 354–90).

There are insufficient data on vessel size to surmise whether the Israelites understood the concept of measure. There are some weights: a quartz weight from Bethel (#281 PTS2-0056), stone weights from Tell Dothan (e.g. #2332), a hematite weight from Tell el-Farah North, a stone weight with possible *aleph* inscription (#3691 ST318) from Gibeon, and a hematite weight (Reg. 3144, #315) from Shiloh. Also, from Shiloh, Area C, a hematite weight was found in the shape of a snakehead (Reg. 3144, #315). The weight is 135.3g, which is exactly the Iron I “Egyptian gold standard” (Eran 1994, 152). In this period, “silver was no longer the money of account as it had been earlier, rather, gold was the major currency” (Snell 1997, 73). A bronze ingot from the “Mount Ebal” site (El-Burnat Sitti Salaamiyya) and bronze slag-encrusted copper-making crucibles from Khirbet Raddanah may also relate to standardization, as a late 12th-century Assyrian text lists tin and bronze used as currency (Freydank 1982, 64–75). Of course, since these are all in response to trade, it need not be said that the *Israelites* understood the concepts of standardized weights, accounting, and finance, only that their trading partners did.

Iron I texts could also be included under cultural artifacts. These are minimal—the Izbet Sartah abecedary (Demsky 1977, 14–27), a potsherd from an Iron I burial cave at Manahat inscribed *lšdh* (Stager 1969, 45–52), and a potsherd inscribed *šmn*, “oil,” from the tiny village of Khirbet Tannin in the Tell el-Farah North system (Lemaire 1985, 14). The

28. Compare the entirely social definition of ethnicity used in Anfinset 2003, 57–59.

29. On the difference between Q- and R- methods, see Whallon 1972, 13–33 and Voorrips 1982, 93–126.

30. The exception, although undertaken when working outside the Levant proper, is Van Beek (1969).

"Ahilud" inscription from Khirbet Raddanah appears now to be a find without stratigraphy, quite possibly not from Iron I, and should not be considered (Lederman 1999).

This is in marked contrast to the scribal cultures of the LB and Iron II periods, and to continued scribalism at neighboring Aphek, Beth Shemesh, Taanach, etc. (Byrne 2007, 2–3). Ryan Byrne concludes that scribalism survived from LB to Iron II through elite patronage, private instruction, and as a luxury commodity during Iron I (2007, 3, 6, 22–23). This probably underplays cases where writing was reserved for a ritual function (Sherzer 1990, 123) and bypasses the trend in similar societies for literature to be oral even among the literate (Keesing and Keesing 1956, 168). The most curious factor is the restriction of written texts to small, obscure sites: Izbet Sartah, Khirbet Tannin, and Manahat. Why no inscriptions from Shechem, Dothan, Shiloh, Tell el-Farah North, Ai, Bethel, Gibeon, Gibeah—if scribalism was, indeed, an indulgence of the elite?

Religion

"What we would designate *religion* was present in the cultural practices, predicated on shared meanings of land and kinship, that shaped the social occasions of early Israelite settlements" (Meyers 19978, 40). But we can also be more specific, focusing on the architecture of special places and the artifacts associate with such places.

The "Migdal Temple" of Shechem is quite possibly not from the Iron I period (Stager 1999, 228–49; Zwickel 1994, 76). The "altar" on Mt. Ebal may be cultic. There are too few cooking pots and too many jugs and jars for a simply domestic occupation (Gilmour 1995, 111). The percentage of fallow deer bones (10% overall) is high, and 63 percent of these were found in the "altar" (Horwitz 1986–87, 175). Half the bones are cranial bones, meaning the antlers were being especially sought. Of all the burnt bones, 81 percent were sheep and goat.

There may have been a Secondary Public Cult Room at Ai. At the end of a street in Site D there is tiny room #65 (8.5×2.5m), with a bench at the foot of its wall all the way around, where a four-story, 80cm tall fenestrated incense burner with lions' paws as a base was found. Analogous pieces are Megiddo P6055 and others, two at Tell Qasile, and three at Beth-shean. Inside the burner was a clay animal figure (#1091) of either a greyhound dog or a jackal (not a mouse), similar to one found at Taanach. Also in the room were a bowl for a cult stand, having a chalice profile with a tang (#1054), a bovine figurine, and a bowl (#1055) with flat base and a row of breasts around the carination.

The site of Dhahrat et-Tawileh, or "Bull Site," could be an Iron I shrine (contra Dever 2005, 464). One problem is its cultic nature. The pottery and the flint assemblages are those of domestic habitations (Mazar 1982a, 41), although its incense burner (see below) is cultic. A second concern is the matter of its dating. Although the excavator³¹ always referred to the site as single-period Iron I (Mazar 1982a; 1982b, 135–45; 1999, 146), an independent survey of the site found only *five percent* of the sherds collected to be Iron I (Zertal 1996, 2:169).³²

Historically, the main impulse for considering Shiloh a "religious center" was the biblical tradition (as per Faust 2006, 20). There is no excavated Iron I structure that is clearly religious, although large portions of the Iron I site have not been excavated, and excavations of the western side of the tell, near the later church that have just begun have produced Iron I material (Oded Lipshitz, personal communication). Shiloh has produced cult stands (see below).

Overall, there is meager evidence for cultic places.³³ This is especially clear in comparison to clearly cultic Iron I buildings at Dan (Room 7082), Tell Abu Hawam (T.30), Beth-shean (ST and NT), and Tel Qasile (XII, XI, and X). But these are all outside of the central hill country to which many scholars would limit early Israel (cf. Gilmour 2006).

As for cult objects, the famous "bull" of the "Bull Site" was not found by archaeologists but was a chance discovery by a soldier and should not be included (Mazar 1982a; contra Dever 2005, 464).³⁴ The site (Dhahrat et-Tawileh) did produce a ceramic incense burner (if analogous to ones from Megiddo and Beth-shean) or a model of a shrine (if analogous to one at Tell el-Farah North)—unless these are Middle Bronze. Tell el-Farah North silo 249 produced a similar clay model of a shrine or temple enclosure building,³⁵ and also found on the site was a silver and bronze "Hathor" statuette (#1.491). From Shiloh, there are two rectangular cult stands.

31. The entire excavation of this site took one day (Mazar 1997).

32. If we accept Finkelstein's redating of "Einun" pottery to the Middle Bronze Age, then 90 percent of the pottery found at the site is Middle Bronze (Finkelstein 1998, 97).

33. On the lack of Iron I cultic sites in Benjamin specifically, see Langston 1998, 72–179.

34. An additional reason for not considering the bull is the possible redating of most of the site's pottery to the Middle Bronze Age. Parallels to similar bulls from MB contexts could assign the Bull Site's bull to that period, as well (Finkelstein 1998, 97; but cf. Mazar 1999, 146–47).

35. I do not consider it valid to interpret the open space, where presumably a divine image could be inserted, to be "empty space aniconism" representing the invisible Yahweh, as does Dever (2005, 468).

There are some Iron I figurines. Two figurines of horses' busts come from Bethel (#1054 and #1112), and a human figurine (#328 from locus 44) and "Astarte/Anat/Athirath/Asherah" plaque (#104) were also found there. An identical plaque was found at Dothan. From Dothan, too, come two animal figurines and an anthropomorphic lamp (#P1344) with a prone male human figure with arms outstretched as if flying, wearing a crown of five clay globules, stuck on the underside. There are no parallels. It is 16cm long, made with applied clay in the imitation of a Tridacna shell. Dothan also produced two jugs shaped like bulls (#P1237 and #P1232). At Ai was found a figurine of a goose 7.5cm long, similar to ones at Beth-shean. The Iron I seated god figurine from Hazor, stratum XI, is too distant from the central highlands for inclusion (Ahlström 1970–71, 54–62).

Some conclusions obtain. There was very little in the way of permanent cultic facilities in early Israel, if these existed at all. There is possible evidence of altars for the sacrifice of caprovids and a ritual use of deer antlers. Ethnographically, antler rituals are tied to shamanistic views of nature as controllable and not dangerous (see above; Fletcher 2001).

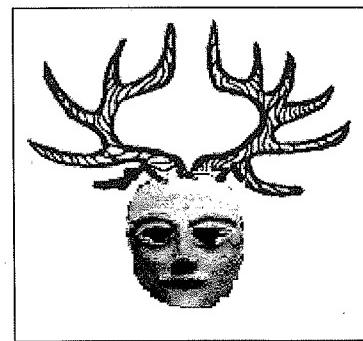


Figure 3. Shamanistic use of antlers.

Adapted by the author from LB Hazor Mask and Horwitz (n.d.).

Incense burners were more common. Although it is possible these incense burners have nothing to do with religion—smudges with hot coals underneath can provide a lot of smoke for many hours or days to repel mosquitoes and other insects—their decoration suggests use in "smudging," cleansing places, persons, or objects of negative spiritual energies and inviting benevolent spirits (Pflüg 1996, 501, 509; Mullin, Lee, Hertwig, and Silverthorn 2001, 20–22). If it is true that, "From the form and style of idols, for instance, we can get glimpses of a society's conceptions of their deities' actions or functions" (Ahlström 1984, 117), then no aniconism is evident, although all statuary is markedly small.

Humans, dogs (the Egyptian Mates/Beba? Anubis?), horses (there are no Egyptian or Canaanite horse gods; the nearest are Hittite; Ivanov 1999, 161–213), and geese (the Egyptian Amon? Seb?) are all represented, with a slight preference for female humans (including some form of Anat/Astarte) and, notably, no good evidence for bulls or calves other than the two bull jugs of Dothan (in contrast to a later period's "Calf Yahweh" from Samaria Ostracon #41). This "pantheon" does not accord well with either the biblical text or Rameses III's mention of only Anat/Astarte and Baal among Levantine gods (Hirsch 2001, 223–27).



Figure 4. Egyptian Goose Deity.
Source: Maspero (1903)—public domain.

We should avoid labeling this "popular religion" (as does Ahlström 1984, 118), a term rightly rejected altogether (Davis 1982, 322; Nunn 2006, 58). Is the designation "popular" used because this was the religion of the common person or because it shows non-normative local particularity (Chartier 1984, 230, 233)? "Religion must always be studied as the religion of certain groups in a given time and place" (Davis 1982, 322).

The catalogue of mortuary evidence from the Iron I highland settlement is meager. Sufficient excavation has taken place now to say that it is *too* meager, and that graves should have been found (Faust 2004, 174–75). If the dead were disposed of by exposure, bone scatters would have been found (Faust 2004, 175). This means the main disposal method was simply inhumation outside of settlements.³⁶ Raz Kletter attempted to

36. Elizabeth Bloch-Smith (2004, 87) disputes this conclusion of simple inhumation because no simple inhumation burials have ever been found in the Iron I countryside. But the great statistical unlikelihood of coming across such skeletal remains unassociated with architecture negates her argument.

elucidate this practice by the systemic, structural explanation of an “egalitarian society” (2001, 39). Even if this were not clearly contraindicated by residential hierarchy and other data (Miller 2004a, *passim*; Bloch-Smith 2004, 87), there would have to be a cultural explanation. Avi Faust believes it attests to an ideology “of simplicity” (2004, 180, 183). One suspects this is drawn from the biblical text, but it is true that grave goods *are* a metaphor for a particular kind of ideal and not biographical information about the deceased (Whitley 2002, 217, 219–20, 223, noting examples of women and children buried with weaponry in ancient Greece). If not an “egalitarian ideal,” we can at least conclude that the early Israelites had no mythos of a “great warrior” (Whitley 2002, 227).

My earlier analysis found that the tombs of the central highlands fall into two groups: pottery-only graves and others (Miller 2004a, 69–73). Tell en-Nasbeh Tomb 29, Jelamet ‘Amer, Taiyiba, and Tell Dothan’s Khirbet en-Namleh cemetery all have nothing but pottery. Tell en-Nasbeh Tomb 54, Khirbet Nisya Tomb 65, Tell Dothan Tomb 1, Level 1, and Tomb 3 have many additional types of grave goods. The addition of other Iron I tombs catalogued by Bloch-Smith does not change the overall picture (2004, 79–82). There is nearly a double correlation of this division with tomb location: while some of the pottery-only tombs are located in major cities, the graves with multiple types of goods all are attached to sites. This dichotomy between pottery-only tombs, isolated or urban, and city graves with higher numbers of artifact types (“NAT”; Hedeager 1992, 101, 103) could have any number of reasons—cultural or social. We cannot suggest that the elites were buried in the towns and the poor in the country, given the Cypriote Black-on-Red Ware from the isolated pottery-only grave at Taiyiba, and Tell en-Nasbeh’s pottery-only Tomb 29 beside NAT=8 Tomb 54.³⁷

I remain at a loss to interpret these data, even culturally, although in comparison with the Late Bronze Age, surely “changes in burial practices...suggest a shift in values” (Burke 2001, xxiv). We may also note a decline in infant store-jar under-the-floor burials, compared to earlier periods (Hallotte 2001, 37). There is ethnographic evidence for simple inhumation indicating a lack of rites of passage, an abbreviated “liminal phase” (Brück 2006, 297–315) and evidence to the contrary (Morris 1987, 34). Simple inhumation does mean there was no need to avoid the pollution of the earth, and regular burial outside of settlement areas precludes any value placed on visiting the dead.

37. Tombs from Gibeon and Tell el-Farah North also seem to be pottery-only; see Bloch-Smith 2004, 80, 82.

Lastly, we turn to “interpretive communities of the ancient” Israelites themselves. Critical use of the Bible as a historical source (Miller 2003b; 2006, 159–61; see also Burke 2001, 85) “does not mean that there are no materials which the historian could use in these traditions” (Soggin 1978, 49*). “The point...is not that they are worthless, but that they are distorted, and distortion can to some degree be allowed for—indeed, it is the historian’s traditional business to do so” (Burke 2001, 78). “It will be foolish and impossible...to reconstruct historical processes and structural relations from archaeological material alone” (Janssen 1990, 405).

Several observations can be made. First, “the narratives depicting the various ages display diversity and contain ample indications of religious practices and popular concepts that do not accord with the stricter, more standardized manner of later times” (Kallai 2006, 136). Second, the incest regulations of Lev 18 and 20 reflect a “set of proscribed relationships indicative of a family group meeting the labor requirements and social roles of the *Iron I* period, rather than of any subsequent time” (Meyers 1997, 18; *italics added*). Third, the cultural ideology of Iron I would have required genealogies that “weave a comprehensive fabric of kin relationships for all the family households” (Meyers 1997, 37), just as the highly fictive biblical genealogies do. Fourth, a song like Judg 5 and the Bible’s general lack of mythic “hylophobia” (fear of the wild outdoors) may represent the kind of “naming and taming” of the landscape employed by early Israelite pastoralists (Sorenson 1986, 32). In other words, Iron I social conditions could be “the attitudes and values of which works of art [or the religious pluralism, purity laws, genealogies, songs] are the symptoms” (Burke 2001, 79). Yet these notes cannot be moved beyond the plausible, unless one were to eliminate all other possible periods as reasonable *Sitzen im Leben* for these texts (Miller 1998).

Conclusions

“We should not forget that ‘culture’ is a clumsy term, which by gathering up so many activities and attributes into one common bundle may actually confuse or disguise discriminations that should be made between them” (Thompson 1993, 13; Burke 1990, 183). What follows is a working proposal, a plausible past that may approximate truth.

The tribal boundaries and other territorial indications the Hebrew Bible gives for early Israel do not reflect realities from the Iron I period. They come from the work of later Israelites in redrawing the boundaries around their communities for ideological reasons, which are beyond the scope of this essay. At the same time, the bailiwicks around Iron I Dothan, Tirzah, Shechem, Shiloh, and “Benjamin” do not represent

distinct cultures. Instead, evidence suggests a sharing of specific meanings of stylistic signs and a greater grouping of people who recognize those relations among signs. Moreover, in spite of a myriad of aspects of continuity from the Late Bronze Age, the new Iron I highland settlements and settlement patterns indicate new, different reactions to the cultural meaning imposed on the landscape by its Late Bronze Age inhabitants.

The early Israelites held no strict dichotomy between domestic territory and open space. On the contrary, they had ways of becoming more familiar with the landscape, such as assigning place-names or composing songs about the landscape, along the lines of such folk songs as *Judg 5*.

The culture cannot be called patriarchal without qualification. Female agency and assiduity were substantial. Parental interaction with children, however, required strict authority, presented as divine mandates and maintained by harsh penalties, household ritual activities, family storytelling, and traditions like the incest regulations of *Leviticus* and the biblical genealogies.

Early Israel understood the ideas of standardization of weights, probably in relation to neighboring peoples. Writing was also known, although not widely used. It may have served a ritual function. It is questionable whether writing was the object of elite patronage, private instruction, and a luxury commodity, given the restriction of surviving written texts to small, obscure sites, for which I have no explanation.

The early Israelites encoded no ideal of martial prowess in their burials. They did not shun internment out of veneration for the earth. They placed little value on visiting the tombs of the deceased. The dichotomy between pottery-only tombs, some isolated in the countryside and some urban, and graves with higher numbers of artifact types found only in towns, cannot yet be explained.

Early Israelite culture saw nature as controllable and safe, wholly in keeping with use of illustrative material from nature in the Psalms and in the wisdom literature.³⁸ Moreover and perhaps related, early Israelites did not construct permanent cultic facilities of durable material, large statuary, or altars of any great size. The exception, on Mt. Ebal, attests to sacrifice of sheep and goats and ritual use of antlers, again consistent with this view of nature.³⁹ Incense burning was a part of the religion,

38. E.g. Pss 11; 42; 61; 71; 72; 89; 96; 98; 121; 147. Note the “naming the landscape” in Ps 89:11–12. Malevolent nature is found in Ps 123 (see Carver 1931, 398–409; Grossberg 1998, 70–71; but cf. Blidstein 1964, 29–36; Grossberg 1998, 72–80).

39. If the Bible is to be compared, perhaps we must consider not merely “Joshua’s altar on Mt. Ebal,” but the possible existence of a northern “Bethel” near Shechem; see Garcia Treto 1967.

used in some form of communication with the spirit world. The biblical depiction of early Israelite religion is “accurate” in its ample indications of religious practices that do not accord with the stricter, more standardized religion of later times. Divine images, all small, are both anthropomorphic and zoomorphic (but not therianthropic), reflecting (or at least responding to) the Egyptian and traditional “Canaanite” pantheon. Nevertheless, there are no clear images of any bovine deities, nor of Baal, El, or, indeed, Yahweh.

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DAVID THE MERCENARY

John Van Seters

Introduction

The search for the historical David and the efforts to reconstruct the so-called United Monarchy has given rise to a proliferation of monographs and studies, both archaeological and literary. There would of course be no such archaeological search if it were not for the literary record in the Hebrew Bible, the Iron I stratum of Jerusalem being absolutely mute, so that everything rests upon how one criticizes and evaluates the literary tradition historically. For a long time now biblical scholarship has, as a general rule, regarded a very large portion of the Davidic corpus as either based upon a written literary corpus or archival record to be dated within a few years or decades of the events that it portrays, or at the least stemming from traditions transmitted from those early days. This is particularly true of those works that Leonhard Rost (1982) characterized as the History of David's Rise (HDR) and the Succession Narrative (SN), which have been consistently interpreted as apologies for the legitimization of the reigns of David and Solomon, composed by members of the court of these two kings.¹ We are told that one needs merely to read these biased accounts of real events “against the grain” to uncover the social and historical reality of those early times.² Even when it is pointed out that there are numerous anachronisms scattered throughout these works, the response has been to blame these on “redactors” and to bracket them with little effect on the whole. What has been lacking in the discussion up to this point is to find a way by which one can fit a rather large portion of the whole corpus into a historical and social framework that can decisively fix the context of the author(s) in his or her own day. This can be done by looking more closely at the institution of the army throughout the Davidic corpus, and in particular the use of professional soldiers and mercenaries.

1. For an extensive review of earlier research, see Dietrich and Naumann 1995.
2. For example, see Halpern 2001.

Within the story of David there are abundant references to mercenaries. This applies to the role of David as the leader of a mercenary band, as well as to David's use of foreign mercenaries after he became king, as a regular part of his military force. In addition there are references to other nations using mercenaries against David in his wars with foreign enemies. All of this is quite significant because, as we shall see in the discussion that follows, it is extremely unlikely that there was any widespread use of mercenaries at such an early date, there are very few references to mercenaries in the later monarchy and no clear evidence that any other kings of Judah and Israel made use of them. The few references that one does find to mercenaries in the rest of Kings are quite suspect as anachronisms. It would appear to be the case, therefore, that the references to mercenaries within the story of David reflect the time of the author of such narrative accounts and not some ancient tradition. Thus, if we can gain some clear idea about the rise of the phenomenon of mercenary use in the Near East, then we may be able to use this understanding of the role of mercenaries to date the narratives in question and perhaps to distinguish those parts that refer to mercenaries from those that do not. What I hope to show is that the abundant references made to mercenaries give us one of the best controls over the date and extent of the source in which they are found.

The Use of Mercenaries in Greece and the Near East

The Assyrian Period

There is an extensive body of literature on the military use of mercenaries from antiquity to modern times with a fairly clear picture of when and how this phenomenon arose, the social conditions that produce mercenary soldiers without a state and the need for states to employ such professional soldiers within their military institutions.³ The conditions that resulted in a widespread use of mercenaries in the Near East may be said to have arisen with the Neo-Assyrian empire. Some scholars do begin with the time of David, but that is because they accept the texts of 1 and 2 Samuel as historical and contemporary with David.⁴ This is

3. No exhaustive bibliography will be attempted here, but the following major studies will be cited: Parke 1933; Griffith 1935; Adcock 1957, 19–25; Garlan 1975, 93–103; Redford 1992; Yalichev 1997; Waterfield 2006. In the early modern period one finds extensive use of mercenaries during the Thirty Years War; see Wedgewood 2005.

4. Yalichev 1997, 62–69. Yalichev also deals at length with mercenaries in the 3rd and 2nd millennia B.C.E., but that is not relevant for this study.

problematic for the many reasons presented in this study. So we will begin with the Assyrians.

Prior to the time of Tiglath-pileser III, the Assyrian army was made up of militias of part-time soldiers recruited from the local population of the various provinces and regions (Yalichev 1997, 70–79). With Tiglath-pileser this changed into the professionalization of the army by using a large central highly trained army as the backbone of the force. This permanent army was committed to absolute loyalty to the king and greatly increased his control of the realm. Plunder from military activity was the primary means of maintaining this system, which encouraged the pattern of annual campaigns and the continuous growth of the empire. The need for increasing manpower to sustain this force was met by the deportation of large elements of the population, among which were trained soldiers who could then be used in other parts of the empire for military and garrison use. These foreign forces were not mercenaries in the usual sense of employment of bands of military adventurers under contract with their own leaders, but prisoners of war who were coerced into service under the victorious regime. Thus, the Assyrians could add to their own citizen army the best of the troops that they conquered, such as in the cavalry corps, which became mercenaries, not by choice so much as by necessity. Only in special circumstances did the Assyrians hire foreign troops as auxiliaries, such as the Sythians, known as expert horsemen. In order to pay for the large professional army of native soldiers and foreign mercenaries the Assyrians began the habit of campaigning on an annual basis. Towards the end of the Assyrian empire, the economic chaos that was created by the constant invasion of armies created increasing numbers of desperate bands of men who were willing to submit to Assyria in return for a regular meal.

The Saite Period

The decline of the Assyrian empire in the mid-7th century permitted the recovery of Egyptian autonomy and the rise of the 26th Dynasty of Saite rulers, beginning with Psammetichus I (664–610 B.C.E.) who was able to unite much of Egypt once again under his control. He then entered into an alliance with Gyges of Lydia (655 B.C.E.) who sent him mercenaries from Ionia and Caria that were under his control, and thus the Greek mercenary first arises not as an aspect of warfare in Greece itself, but in foreign service (see Redford 1992, 433, 441). It was acknowledged that the Greek infantry was superior both in terms of its use of heavy armour and its deployment of such troops in battle, so that Egypt under the Saites was among the first to employ such hoplite soldiers from Ionia and Caria as mercenaries (Parke 1933, 4–6). These Greek mercenaries

were stationed in fortifications in the eastern Delta, most notably at Daphnae on the Pelusiac branch of the Nile and later Greek settlers and traders established a permanent colony at Naukratis in the Delta. Herodotus (2.114.3) claims that Apries had 30,000 Ionians and Carians under his employ. They also served as the Pharaoh's personal bodyguard in the Saite period. The Greek mercenaries played a major role in the final struggle of Egypt against Cambyses and the Persians, but with the Persian victory the employment of Greek mercenaries in Egypt came to an end (ca. 525). It was only much later, after the Greek-Persian War that Greek mercenaries began to be used by Persian satraps and rulers.

The unity of Egypt under a single administrative control and the encouragement of foreign trade with Greek states and the Phoenician coast brought great prosperity to the Saite rulers who were then able of hire mercenaries from many different regions, including not only Ionia but also the Levant and Syria. Military camps of Greek mercenaries, most notably at Daphnae, were set up in the eastern Delta, and Greek garrisons were also set up on the Egyptian frontier. Note the presence of the Jewish mercenary colony of Elephantine. In addition to the control of Egypt the Saite rulers also attempted to extend their rule over the Levant as far as Phoenicia and establish provincial control of the region with the local rulers as vassals. Military control of this region was largely in the hands of mercenaries. As Redford explains:

In the event Saite Egypt became a highly desirable employer for Asiatics as well as Greeks. Syrians came to Egypt and are found in communities at Migdol, Athribis, Memphis, Thebes, and Aswan, and at special encampments in the Delta... It is clear, then, that the prime function of all these enclaves was paramilitary, although nothing prevented any of them from engaging in commerce. They were organized into "garrisons" and paid salaries from the royal treasury. (1992, 443)

Redford further argues for the extension of this same military policy into Asia. He states:

In the context of Psammetichos's reassertion of control over the Levantine coast, the mercenary troops he had acquired were also used to garrison the strongpoints. One such post has come to light through excavation at Mesad Hashavyahu on the coast not far from Ashdod. Here Greek pottery of the period 625–600 B.C. attests the presence of Hellenic occupants... Southeast, similar pottery has come to light at Tel Melah in the Negeb. (1992, 444)

Redford also interprets a reference to *Kittim* ("Greeks") in the texts from Arad belonging to this same period as Greek garrison troops from Egypt, rather than Greek mercenaries employed directly from their

Aegean source by the Judean king. With the rise of Babylon the control of the Levant was contested between Egypt and Babylon, a struggle in which Judah was caught in the middle to its great detriment. Babylon was not able to conquer Egypt or to subdue entirely the Phoenician cities of the coast. The mercenaries, as the backbone of the Saite military strength, allowed these kings to survive until the conquest of Egypt by Cambyses in 525. However, it must be emphasized that acquisition of a mercenary force and the retention of a standing army in the 7th and 6th centuries, as reflected in Assyria and Egypt, is a very expensive proposition. It is highly unlikely that the kings of Israel and Judah made any extensive use of mercenaries through the period of their monarchies. This assertion may appear to be contradicted by the mention of Carians as part of the palace guard in 2 Kgs 11:4, 19 (see Finkelstein 2002, 149). The reference, however, is clearly anachronistic for the 9th century and must represent the invention of this episode in the 6th century, when Carian mercenaries were being used as part of the bodyguard of the Saite pharaohs. This biblical episode cannot be used to prove that the Judean kings actually did make use of Greek mercenaries, only that such a practice was known at the time of writing.⁵

The Rise of Mercenaries in the Greek World

Garlan describes the Greek mercenary as follows: "The mercenary is a professional soldier whose behaviour is determined not by his membership in a political community but by the lure of profit. The combination of three characteristics—being a specialist, an expatriate and a wage-earner—was peculiar to this type of man in the ancient world as in the modern" (1975, 93). The source of the best mercenary soldiers in the Eastern Mediterranean world was Greece. In this region there were always professional soldiers with specialties requiring training, drawn from certain areas, such as archers from Crete. In the archaic period before the wars with Persia the number of mercenaries in Greece was quite small, serving primarily as personal bodyguards for tyrants. It was only in the rather special circumstances of the Saite deployment of Ionian mercenaries that significant numbers of Greek mercenaries were used in the Mediterranean region. The development of a reliance on Greek mercenaries, especially the Greek hoplites with heavy armament (as portrayed in Goliath's weapons), was particularly important in the evolution of warfare in the Mediterranean region. The diffusion of

5. The history of the use of Carians as palace bodyguards in the Saite period is therefore a fairly secure means of dating the whole episode in 2 Kgs 11 as a literary invention of the exilic period.

mercenaries and Greek colonization in the eastern Mediterranean went hand in hand during this period until ca. 525, at which time there was a hiatus in mercenary activity for about a century.

As Garlan points out (1975, 95–98), a major factor in the use of mercenaries by any city or larger political entity is the ability to pay the wages of the soldiers. In the older period of Greek history, wars between city states were basically conducted by citizen armies, and those states, such as Sparta, whose whole way of life depended upon the development of its citizens in the art of warfare, obviously had an upper hand and continued to be famous for their excellence in warfare. In the great wars against Persia, it was primarily a coalition of citizen armies of individual states against a superior power. These were primarily wars of defense in which service was limited to the length of the campaign and the citizens themselves bore much of the expense of equipment and supplies. It was only certain rich states, such as Egypt under the Saïtes, who could hire Greek armies with special expertise in battle tactics and superior military equipment, and in such cases mercenaries became a major factor in the conduct of war.

Payment for service was a constant source of difficulty between mercenaries and their employer, whether king or tyrant, and delays in payment could lead to revolt with serious consequences for the state involved. The need for guarantees of remuneration led to the practice of formal contractual agreements similar to that of treaties between foreign powers. Thus the arrangements of remuneration for service were laid out and guaranteed by a solemn oath sworn by both parties.⁶ Another way of trying to insure loyalty was by means of land grants. Such military colonization is already known in the Saïte period of Egypt and continued into later periods as well. It could involve the founding of new settlements and the integration of such foreigners into the realm. It gave these landless adventurers property and a stake in the realm which they did not have when they took up their military career in the first place. Of course, the mercenaries also shared in the booty which resulted from specific military campaigns as well as direct payment for service that was stipulated in the agreement.

The social causes of the mercenary system resulted from the impoverishment of many small land-owners who sought relief through colonization and military adventure. The rise of the rule of tyrants also created a need for private bodyguards loyal only to the tyrant who could assist him in the control of the populace that he ruled. These conditions were particularly in evidence in both the archaic period as well as in the time

6. See Garlan 1975, 96–98, for examples.

from the late 5th century onwards. It was, however, not only or primarily the demand of tyrants and foreign rulers but primarily the abundant supply of many soldiers who had experienced war and who, when the war came to an end, had no other means of livelihood. Citizen armies, consisting of farmers, land owners and artisans, could not tolerate long campaigns without suffering financial ruin. Prolonged warfare thus created a great flood of mercenaries from various parts of the Greek world, especially after the 30-year Peloponnesian War (431–404 B.C.E.).

This conflict created the long “habit and discipline of war” (Griffith 1935, 4) as well as the need for a continuous supply of soldiers. And the more the war was prolonged, the more the land was impoverished, which left little else but to earn a livelihood as a soldier in further conflict. This in turn gave rise to a heavy dependence upon mercenaries in the conduct of war from the late 5th century onwards, both in Greece itself and also in the use of Greeks and non-Greeks in Asia.

The Persian Period

After the Peace of Callias (449 B.C.E.) that ended the hostilities between the Persians and Greeks, and following the Peloponnesian War, Greek mercenaries again become employed in great numbers in Asia. The most notable example, among many, of the employment of Greek soldiers in foreign service is the more than 10,000 Greek mercenaries that were used by Cyrus the Younger (401–400) in his failed attempt to gain the throne of Persia, as well as the Greek forces that were also employed by the Persian king, as recorded in great detail in Xenophon’s *Anabasis*. The later Persian kings as well as the satraps of the western satrapies made abundant use of Greek mercenaries down to the time of Alexander. Alexander the Great made extensive use of mercenaries, not only in his war against the Persians but also in the establishment of military colonies throughout the Asian lands that he conquered, and this practice was continued by the Ptolemies and Seleucids. The distinction between these two periods, the first in the 7th and 6th centuries and the 2nd from the late 5th century onwards, is important and can be seen most clearly in the increasing specialization in military personnel between the heavily armed hoplites, who formed the main body of the assault troops and the light-armed auxiliaries, the archers, slingers and spear-throwers. In addition to these foot-soldiers was the cavalry. Among the light-armed troops, most effective in skirmishes and swift attacks, were the Cretan archers and the *peltastai* of Thrace, known by their use of the small shield ($\pi\epsilon\lambda\tau\eta$). As Griffiths points out, the end of the 5th century gave rise to a different kind of warfare, that of prolonged skirmishes in a warfare of attrition,

for which the light infantry of the mercenaries were best suited, rather than the citizen hoplites used in a single decisive battle. Thus it was precisely the light-armed peltast who most represented this form of mercenary and who played an increasingly important role in the 4th century. So ubiquitous is the mention of the peltasts in the references to mercenaries in the second period and so completely lacking are they in the accounts of the earlier period that it becomes an easy marker of texts that use this reference to Greek mercenaries.

Regarding the peltasts, Adcock notes that these rather barbarian Thracians were especially known for their skill at spear throwing and their brutality. Adcock (1957, vii, 29) refers to Thucydides, who gives a vivid picture of the raiding and massacre of small communities by such bands of mercenaries: “The Thracians burst into Mycalessus, sacked the houses and temples, and butchered the inhabitants, sparing neither the young nor the old, but methodically killing everyone they met, women and children alike, and even the farm animals and every living thing they saw” (Warner 1972, 495; see Adcock 1957, 21). Such groups tended to fight in small battle formations under their own professional leader.

Furthermore, Adcock makes the point that mercenary light-armed troops could be used more easily than regulars in a variety of terrain and conditions and they could also garrison strongholds and fortified positions away from the city centers, which were the homes of the citizen armies, because they were basically homeless. Citizen troops fought during a particular campaign season and then returned to their homes and regular occupations. Mercenaries had no local patriotism and therefore no political attachments and thus were most useful in the service of tyrants and foreign kings. Their loyalty was entirely to those who paid them. It is also the case that the generals of mercenary forces, with their widespread foreign experience in many different situations and their continuous engagement in military activity, became superior to the traditional generals of national armies and their conservative tactics of warfare. This even happened in actions against the vaunted Spartan hoplites (Adcock 1957, 22–25).

In his book, *Xenophon's Retreat*, Robin Waterfield gives us a vivid and detailed view of the role of Greek mercenaries with the Persian armies at the end of the 5th century B.C.E. Regarding the insurrection by Cyrus the Younger, he states:

In all, Cyrus hired some 10,600 Greek hoplites, and about 2,300 peltasts. They came in several units, depending on their ethnic loyalties and on their recruiting officers, and each soldier's first allegiance was to the fellow members of his own unit and to his commander, who secured his

men's fickle loyalty by being the channel through which pay from Cyrus would reach them. Until they were forced by the defeat at Cunaxa to unite against a common enemy, relations with other Greek units tended to be tense and divisive (and the Greeks as a whole also kept themselves aloof from the Persians and other non-Greek troops—but then, members of elite regiments have always acted with haughty disdain towards other units). (Waterfield 2006, 79)

Included among the peltasts was a small contingent of two hundred Cretan archers. The latter become increasingly important as mercenaries in the Near East, and Griffith states that, “By far the most prominent Greek soldiers throughout the Hellenistic period were the Cretans” (Griffith 1935, 245).

As Waterfield indicates, both the Greek officers and their rank and file were “an unsavoury bunch,” “hardened fighters” who were looking for a big payday from Cyrus, if they won, and the chance of “marauding their way back again afterwards.” The reason why the Persian leaders employed these “thugs” is that they were simply the best warriors; as such, they were in great demand as such. Even after Cyrus was defeated, the battle “advertised to the Persians the value of Greek mercenaries, and both satraps and kings used them in increasing numbers in the fourth century.” And conditions in the Greek homeland were such that the supply from all the different regions of the country was equal to the demand, both in hoplites and in the light-armed peltasts. One of the dangerous social side-effects of this form of employment was that when they returned home to Greece they would either continue their pillaging there or create military tyrannies and provoke unwanted wars for further employment (Waterfield, 2006, 79–81). Wakefield refers to Isocrates’s description of mercenaries as “vagabonds, deserters and criminals, given to preying on others. Unattached mercenaries, exiles from their native cities, formed bands, lived in caves in the mountains of Greece or Asia Minor, and survived by scavenging and brigandage” (2006, 81).

Waterfield goes on to point out: “Since mercenaries worked for pay, it was a common belief, with a solid factual foundation, that they would desert for better pay, or if they calculated that the odds against them were too great” (2006, 81). They lacked the sense of loyalty of the citizen soldiers who had a stake in defending their own community and property. However, increasingly these landowning citizens were inclined to hire mercenaries and “fight by proxy.” Thus the older traditional system of states relying on alliances and allies to supplement their own resources gave way to hiring mercenaries instead. “The fourth century saw amateur soldiering give way almost entirely to the professionalism of mercenaries, whenever there was an important battle or war to be fought”

(2006, 81).⁷ From this extended discussion of the mercenary soldier and his role in the Persian period a clear picture emerges on the rise of this phenomenon in the Near East and the degree to which it reaches a peak in the Persian period, and this allows us to set the frequent references to mercenaries in the David story against this social background.

There is one more feature that is characteristic of ancient warfare in general and mercenaries in particular, and that is the phenomenon of the baggage train that accompanied an army on campaign. Waterfield gives a vivid picture of the baggage train that followed Cyrus's army from Asia Minor to the battlefield of Cunaxa in the region of modern Baghdad (Waterfield 2006, 103–9). He estimates that there were about as many non-combatants as there were soldiers, as well as a large number of animals, both for food and for transport of supplies, in addition to the cavalry horses and the fodder to maintain them. Provisions, equipment, weapons and booty were carried on carts and by slaves; and the amount of goods and personnel that accompanied an army on the move was immense. The farther the expedition and the more prolonged the campaign, the greater the entourage. There were a large number and variety of artisans for cooking food, making and repairing equipment, clothing and weapons; there were personal slaves for officers, wives and concubines for personal sexual needs and women for shared sexual use. During the campaign there was also the accumulation of booty and prisoners, the latter to be sold as slaves. Such a moving city also attracted merchants to sell food and other commodities, as well as to purchase prisoners-of-war for the slave trade.

What makes this understanding of the baggage train particularly relevant for the discussion of mercenaries is that there is an obvious difference between a short war entailing a single battle by citizen soldiers within a small region and in defense of a city, on the one hand, and the prolonged adventurous campaigning by mercenaries with no fixed abode, on the other. In the former case, the baggage train was of much more limited scope and duration, whereas with mercenaries, the military entourage was a way of life. Even in the case of large professional armies, such as the Assyrians and the Persians, there was a tendency to ensure the success of campaigns with provincial and outlying supply centers along the main military roads. But mercenary armies fighting in hostile terrain were much more dependent on the baggage train and on local marauding for all their needs.

7. It is quite remarkable how similar this situation is to the character and behavior of the mercenaries and their use by European states in the Thirty Years War. See Wedgwood 2005.

Mercenaries in the Story of David

David's Career as a Mercenary

There are many direct references to mercenaries and the activities in which such persons engage in the story of David, and once this fact becomes clear then many additional details in the story become explicable as part of this phenomenon in the light of this social context. These references, with one exception, all belong to the same late source, and this will be our primary concern. First, however, we will deal with a reference in the earlier source, which I have attributed to DtrH.⁸

In 1 Sam 22:2 Dtr gives us a description of David forming a band of men over whom he becomes leader. It states: "Everyone who was in difficult circumstances, everyone who was in debt, and every malcontent, gathered to [David]; and he became their commander. About four hundred men followed him." This describes very well the social situation that arose in the late monarchy from Sargon and Sennacherib down to the time of Nebuchadnezzar, as we have seen above, so that David's group reflects a common social feature of this period. These persons lived in caves and other wild regions of the country, living a very hand to mouth existence and often preying on small communities. David and his men, however, are presented in a positive light as saviors of the small town of Keilah, which was under attack by the Philistines (1 Sam 23:1–5). Thus, Dtr suggests that during this period when David is in hiding in Judah from Saul, he is engaged in just this kind of activity. He and his men are not actually employed as mercenaries by anyone but live on booty taken from the Philistines, acting on behalf of the people of Judah. After David becomes king and engages in military activity against the Philistines, the Moabites, the Arameans, and the Edomites (2 Sam 5:17–25; 8:1–14; 10:15–19), there is no further suggestion of mercenaries used either by David or by his opponents. All the armies involved are national armies of citizen soldiers and when they find it necessary, they enter into political alliances with other kings or use troops from vassal states, as in 2 Sam 10:15–19. These accounts all belong to a time when the use of mercenaries was not yet a pervasive military fact of life.

References to mercenaries in the later post-Dtr account of the David and Saul story⁹ are quite numerous, so we will begin with the most obvious, that of David's employment in the service of Achish, king of

8. This source analysis between an earlier source, identified as DtrH, and a later source will be set forth in a detailed study that is in preparation.

9. This literary work, which I designate in my larger study of David (in preparation) as the David Saga, consists of a large portion of what was formerly called the History of David's Rise, as well as the so-called Court History.

Gath, as a leader of six hundred mercenary soldiers (2 Sam 27:1–28:2; 29:1–30:31). David arrives in Gath with all of his men and their entire entourage, since as mercenaries they are stateless (27:2–3), and he reaches an understanding with Achish that in exchange for his service he will accept the border town of Ziklag in which to settle his entourage of soldiers and camp followers, in order not to be a burden on the capital city. Ziklag will thus become his personal domain. In this capacity he serves as Achish's "servant" or vassal. From this base of Ziklag David carries out raids against the local inhabitants of the region, in a fashion all too typical of mercenaries, as we have seen above, in which he engages in totally ruthless massacres of the communities he attacks in order that none can report to his employer exactly what he has done (27:8–12). All of the livestock and goods of the communities thus destroyed become the booty of the mercenaries and hence their wages. When David returns to Achish to report on his activity, he pretends that he has been engaged in military activity against the Judeans of the region, the enemy and rivals of the Philistines and the purpose for which he has been hired, thus completely deceiving his employer. And who is to report otherwise, for those attacked are all dead? Achish does not question David's reports and commends him for his efforts, since mercenaries are notorious for caring little about ethnic loyalties. Greek mercenaries killed other Greeks just as easily as they killed foreigners.

Many commentators on this text find ways to commend David for his activity because he fought against Israel's traditional enemies, the aboriginal peoples of the region, and then deceived the Philistines, both of which were fair game.¹⁰ That suggests that our narrator had much less humane sensitivity than Thucydides (quoted above) to the atrocities regularly committed by mercenary bands, bands which acted purely out of greed and concern for booty. I think that this is not the case. The author repeats the remark about the wanton massacres and says that this was David's custom for the whole period that he was under the Philistines (27:11). The deceit about David's attacking the southern regions of Judah conceals his future ambition to become king of Judah and it leads Achish to trust him and believe that he would always be his mercenary vassal. Given the widespread contempt for the brutality and complete unreliability of mercenaries and their leaders in antiquity, there can be little doubt that the narrator presents David in this same negative light. Furthermore, when Achish invites David to participate in the Philistine war against Israel, David immediately agrees, and this leads Achish to

10. See McCarter 1980, 416. His remarks are typical. See also Edelman 1991, 232–37.

promise that such participation will be rewarded by elevating David to the status of the king's personal bodyguard (28:1–2). Mercenaries were often used by kings and tyrants as the palace bodyguard. Achish reasons that if David is already alienated from Judah and becomes further alienated from Israel in the forthcoming battle against Saul, he will have little choice but to serve under Achish's employ.

When Achish brings David and his mercenaries to the joint Philistine muster (29:1–11), matters are quite different. The various commanders of the other contingents are suspicious of the mercenary leader and his group of Hebrews, and rightly so as the narrator has made clear. The commanders of citizen soldiers are always suspicious of mercenary leaders and quarrels are common in such joint campaigns, as is vividly described in Waterfield's treatment of the expedition of Cyrus (2006, 91–102). The other commanders are fearful of the possibility that David, with his mercenary band, might desert to his former employer in the heat of battle, a possibility that is all too common with mercenaries who have no loyalties to the state for which they are fighting. The "tyrants" of the Philistines have their way and Achish must report the bad news to David that he is not to fight in this battle but to return to his post in Ziklag. The whole discussion between Achish and David is full of irony. Thus David protests his innocence, although the reader knows just how guilty and unfaithful he is—a typical mercenary who is only out for himself. Achish even begins his remarks to David by reciting an oath in the name of Yahweh, which seems to reflect a similar oath made by David to Achish of his loyalty. Why else would Achish invoke the name of David's god? Oaths of personal loyalty, sworn by mercenaries, were very important in lieu of any other guarantee.¹¹

It must also be understood that David and his men are recognized throughout this episode as highly skilled professionals with a lot of battle experience, having been engaged in fighting constantly for a year and four months. They are not just a ragtag group of malcontents. Achish considers them a great asset, while the other leaders regard them as very dangerous. They would not be used in combination with the other soldiers but would fight as an independent group, under David's sole command. In such circumstances there could not be any doubts about their commitment to the Philistine cause, and the other leaders were not willing to take that risk.

David returns to Ziklag only to discover a disaster has taken place in his absence. The Amalekites who have suffered frequent massacres at the

11. For examples of such oaths between rulers and mercenaries, see Garlan 1975, 96–98. The oaths by both parties invoke the same gods.

hands of David's men now retaliate with a raid of their own on Ziklag while David and his men have been far to the north in Aphek, and thus have left the home base unguarded—a very grave mistake (30:1–6). Yet what is quite remarkable is that the raiders, who completely sacked the city, took the entire population captive, but "they killed no one" (v. 2). This is stated in such explicit and marked contrast to David's own behavior, that for the narrator it must be significant. It simply points up the bloodthirsty character of David and his men towards these same Amalekites. The blame for the grievous oversight in not protecting those left in Ziklag is placed upon David to the point of mutiny (v. 6), which is only averted by David's appeal to an oracle that calls for immediate action and full pursuit (vv. 7–10). Without corpses in the ruins of Ziklag it was apparent that the wives and children had been taken captive, no doubt to be sold as slaves, which was a common result of such raids. The oracle by David's personal priest merely stated the obvious, and took David "off the hook."

A number of details in this episode are noteworthy. First, there is the remark about how two hundred of the six hundred soldiers were too exhausted to continue the pursuit (vv. 9–10) and they stayed behind at the Besor river, while the rest continued the pursuit. Yet, on David's return from the raid this group is somehow converted into a guard for the baggage train, which as we know from the earlier discussion is a regular feature of such campaigns (vv. 21–25). It was, of course, a military necessity always to supply a guard for the baggage, and all the more necessary for mercenary armies that traveled like vagabonds most of the time (see also 25:13). The division of the spoil with those who guard the baggage made sense. The saying attributed to David and thus recognized as a statute "to this day" is a quite fictional etiology that simply recognizes a universal practice for this kind of warfare.

In pursuit of the Amalekites David and his men come across an Egyptian who was a personal slave to one of the leaders of the Amalekite raiding party and who was abandoned when he became ill, a not unusual practice of an army on the march. Sick slaves and animals that could not carry their load were expendable. But such a person is invaluable to David in terms of providing intelligence about the activities and whereabouts of the raiding party. One place that is mentioned as having been raided by Amalekites is a community of Cherethites in the Negev (v. 14). This use of Cherethites (Cretans) is often compared with the references to Cherethites in Ezek 25:16 and Zeph 2:5. In these prophetic texts Cherethites is used as a synonym for Philistines, perhaps in a derogatory sense, reflecting an older tradition about Philistine origins in Crete (Amos 9:7). However, that does not seem to be the case in 2 Sam 30:14.

These are Cretans and not Philistines, who elsewhere in this source are Greek mercenaries. The remark, therefore, may have in mind colonies or outposts of Greeks which had been set up by the Saite rulers to control the coastal region between Palestine and Egypt. However, there is no indication that they ever used Cretans for this purpose, so that a Cretan colony in the region is probably a fiction of the narrator.

With the information given by the slave, David is able to find the raiding party and make a surprise attack, which results in a complete recovery of all the persons and goods and much additional booty (2 Sam 30:11–20). As with his former practice, he executes all the enemy combatants except those who escape on camels. This great booty is then brought back to Ziklag and much of it is distributed as gifts to the friends and elders of Judah in the cities of the region for obvious political purposes. It is usual in the case of booty acquired by military means, even by mercenaries on raids, to dedicate a tenth of it to the deity in recognition of divine assistance. Although David also recognizes divine assistance and describes the booty as "that which Yahweh has given us" (30:23), and "the spoil of the enemies of Yahweh" (30:26), no dedication of booty to Yahweh is mentioned. It is clear that throughout chs. 27, 29–30 David is being portrayed as a typical, but also very astute, mercenary leader. As such he is ruthless and deceptive, seeking his own interests and those of his men. He displays no concern for a higher cause or principle, and uses religious legitimization for his actions only when it suits his purpose.

The narrator of David's employment by Achish in 2 Sam 27, 29–30 has not introduced this band of mercenaries *de novo* for these episodes alone, but has already anticipated their existence in the previous episodes that he narrates. David's four hundred men in Dtr have become six hundred men of a well-organized and well-equipped fighting force (23:13; 25:13) within the later narrative. The episode in 2 Sam 25 is very revealing. David and his men demand from one of the rich land owners of the region in southern Judah that he give them "protection pay" for not raiding his flocks. Nabal dismisses David as someone of no consequence and his motley freebooters as run-away slaves. David's response is to have his men prepare for a massacre. It is of interest to see that David's forces are divided into two groups, four hundred who will engage in the actual raid and two hundred who will guard the baggage train (v. 13). This is exactly the same arrangement that we saw above with respect to David's raid on the Amalekites, and suggests that David is already the leader of a well-organized and experienced band of mercenaries. There is basically no difference in David's behavior before or after he becomes employed by Achish.

The disaster is averted by a young servant's recognition of the grave danger and his warning to Abigail, Nabal's wife, who immediately intervenes with a lavish present of food to buy David off. She seduces David with a lot of pious words that appeal to David's ambition as a future king of the region, in which he will have cause to regret such bloodshed within Judah. Her remarks about preventing David from "shedding innocent blood" (v. 31) and of "fighting the battles of Yahweh" (v. 28) are surely ironic when it comes to his subsequent career under Achish. David and his men are mercenaries and as such it is their nature to pillage and to shed the blood of their innocent victims. Furthermore, when David acquires Abigail as his wife he does not settle down on a large estate, because, as Abigail has suggested in her speech, she is aware that he has much greater ambitions and as such must remain a mercenary until the opportune time.

In David's second encounter with Saul (2 Sam 26), in which he spares Saul's life, David's band of warriors and Saul's elite corps of three thousand fighting men are a case of two professional armies confronting each other. The mercenary band, although outnumbered five to one, has a tactical advantage in fighting on its own turf. What is also of interest is that among David's mercenaries are foreigners, such as Ahimelech the Syrian (Hittite¹²), who seems to rank at the same level as Abishai. This is the same ethnic mixture in the army that one will find in the Court History and is entirely typical of West Asian monarchies and satrapies in the Persian period. Furthermore, David's protest about being driven out of the "heritage of Yahweh" to "serve other gods" is rather empty in that as a mercenary he willingly goes to Achish when he is no longer pursued and it is clear from Achish's invocation of Yahweh in 2 Sam 29:6 that David need not serve any other gods but his own. There is no indication that mercenaries ever needed to swear allegiance to any other country or their gods, as did vassal states. As the comparative material makes clear, the arrangement between a mercenary and his overlord was entirely a monetary one.

The Use of Mercenaries in King David's Army

After David receives word that Saul and his sons have been killed by the Philistines, David moves to Hebron, and according to the late David Saga (DS)¹³ he brings up with him the same body of men as his personal

12. The term "Hittite" is used in late texts as a reference to a non-Israelite aboriginal member of the population of Syria-Palestine; see Van Seters 1972, 64–81.

13. See n. 9 above.

army, along with the entire entourage that he had as a mercenary leader and settles them in the region of Hebron (2 Sam 2:1–3). David is then made king of Judah at that time (v. 4), though he continues to be at war with the house of Saul, and his forces are under the same leadership, the sons of Zeruiah, who were with David prior to his coronation (2:12–32). The forces of Saul under Abner are regularly characterized as the "men of Israel" or "Benjamin," while the forces under Joab are consistently designated as the "servants of David," that is, his earlier band who have now become his personal professional army. Furthermore, on the occasion when Abner conspires to transfer the allegiance of all Israel and Benjamin to David in a private agreement, 2 Sam 3:12–25, the narrator states that Joab and the "servants of David" have just arrived back at court "from a raid, bringing with them an abundance of booty." According to the DS narrator, it would appear that they still continued their old habits of marauding and slaughter. Joab, who already has a grudge against Abner for the death of his brother in combat, now is in danger of being overshadowed by Abner, who commands the whole of the forces of Israel and Benjamin, while Joab is only the commander of the army of former mercenaries. So Joab murders him. This would not be the last time that Joab did such a thing. The point that I wish to make from all of this is that such behavior reflects the same social milieu that was characteristic of military life in the Persian period. The narrative is all of a single piece reflecting the same social conditions.

After David becomes king of all Israel he continues to use his professional army to do his fighting for him in DS, in contrast to the earlier DtrH, which always has David leading his forces in battle. However, in addition to his regular professionals, the author of DS also has David employing foreign mercenaries. In both lists of David's officials there occurs the figure of "Benaiah the son of Jehoiada [who] was over the Cherethites and the Pelethites" (2 Sam 8:18; 20:23), and this is in addition to Joab as commander of the regular army. There can be no doubt that the Cherethites and Pelethites are none other than Greek mercenaries, the Cherethites being Cretan archers and the Pelethites being the famous peltasts, light-armed spearmen, so common in the 4th century.¹⁴ So important does the narrator view these mercenaries that Benaiah, their commander, is a serious rival to Joab in the story of

14. The lists in 2 Sam 8:16–18 and 20:23–26 (as well as 1 Kgs 4:1–6), all belong to this late source. The attempt to reconstruct a Davidic "cabinet" from them is misguided. The lists of David's wives in 2 Sam 3:2–5; 5:13–16 likewise belong to this late source.

Solomon's succession, and the one who, as a supporter of Solomon, eventually murders Joab and replaces him as general of the army (1 Kgs 1–2; cf. 4:4). In another tradition (2 Sam 23:23) this mercenary force is also identified as the palace guard, which is most appropriate for the role that he plays in the Solomonic coup of 1 Kgs 1. These same Greek mercenaries also played a role in the Absalom revolt as part of David's loyal forces (2 Sam 15:18) and in the revolt of Sheba (2 Sam 20:7).

In addition to the Greek mercenaries, there is a band of six hundred Philistine mercenaries from Gath under their own commander Ittai (2 Sam 15:18–22). The scene, set at the beginning of David's flight from Absalom, is most instructive on the role of mercenaries in David's army. David acknowledges that Ittai has only recently arrived from Gath as a foreigner and an exile and he suggests that perhaps he should return to his homeland (vv. 19–20). The comparison with David's own former service as a mercenary in Gath is obvious, but Ittai makes a solemn oath of loyalty in the name of Yahweh that he will serve David to the death and he becomes part of David's entourage (vv. 21–22). Note that as a mercenary band all of the dependents must also accompany the soldiers. This is likewise the case for David himself and his men, who are treated as a band in exile. Furthermore, it is noteworthy that in the arrangements for the final battle, one third of the command is placed under Ittai the Gittite, the core of which was undoubtedly his own men (18:1–2). One must also reckon that the Cherethites and the Pelethites were thought of as being under the command of Abishai (cf. 20:7). Throughout the whole account of the war between David and Absalom, the narrator leaves the reader with the clear impression that David's army consists of a core of professionals augmented by a large body of foreign mercenaries. Absalom's army, by contrast, is constantly referred to as the "men of Israel," an army consisting entirely of citizen soldiers. Absalom's force vastly outnumbers David's army but the latter are battle-hardened professionals, so that the Israelite conscripts are no match for them. The casualty figures of twenty thousand is of course totally unrealistic and is derived by imitating the figures that Dtr gives for the dead in the Aramean and Edomite wars (20,000, 22,000, 18,000 in 2 Sam 8:4–5, 13).

At the end of the revolt of Absalom, when David again seeks to reclaim the throne and be reconciled with the elders of Judah and Israel, David seeks to replace Joab once again with Amasa, the general of the joint forces of Judah and Israel under Absalom (2 Sam 17:25; 19:9–14). However, a dispute breaks out between the citizen forces of Judah and the citizen forces of Israel over royal prerogatives (19:40–43), and this leads to a break-away group of Israelites under Sheba who leads a second

revolt. Amasa, the new general of the army, is delayed in his efforts to call up the recruits from Judah at short notice, so that David is forced to rely on his professionals and Greek mercenaries to deal with the situation (20:1–7). When Amasa does show up, he is murdered by Joab and there is little that David can do about it. According to the narrator of this source, the monarchy has become completely dominated by the commanders of the professional army and the mercenaries with the king as a mere figurehead. In his final speech to Solomon David advises his son to get rid of the ruthless Joab (1 Kgs 2:5–6), and he does so (vv. 28–35), but this only leaves Benaiah, the commander of the mercenaries and murderer of Joab, in his place.

This totally fictional account of David's monarchy presents a vivid picture of the militaristic regimes of the Persian period, with an elite professional core and heavy dependence upon mercenary armies, with specialized tactical skills such as those of the Cretans and the peltasts under the control of their own commanders. The old citizen armies were no longer any match for such professional killing machines. Those wealthy regimes, such as Persia, could invest fortunes in buying such protection. The Davidic monarchy is portrayed as a state with such resources, and the author sets out to recreate what life would be like in such an imperial state. His knowledge and understanding of the life of a mercenary and those regimes that make use of them is quite remarkable and realistic when judged against the background of the social milieu of the 4th century B.C.E. There is no other period that provides such an appropriate context for this portrayal of a Near Eastern monarchy than this particular period.

Conclusion

The narrator of this David Saga, which spans both his revision of the rise of David to power and the history of David's court, paints in vivid colors a portrait of David as an ambitious and gifted warrior who creates a mercenary army and uses it to advance step by step to the throne of Judah and Israel as a usurper and then to retain control of the realm by the continuous use of his former band of mercenaries together with foreign mercenaries. So pervasive are the references to David and his mercenaries that they cannot be removed by means of clever surgery, using the methods of redaction-criticism, and attributing them to a redactor. The texts in which the references to mercenaries occur interlock with so many others in this comprehensive David Saga that to engage in such an exercise would result in an *Urtext* of rags and tatters in place of

the finest and most sophisticated piece of prose in the Hebrew Bible.¹⁵ It must likewise be emphasized that this is not a matter of reading the Samuel–Kings narrative “against the grain” to extract historical information from the events portrayed, as if the intention of the narrator is to disguise the real facts by falsely presenting an apologetic and laudatory view of David. None of the episodes that he creates correspond to historical events. His work, in all its magnificent and realistic detail, is fiction, so that what he includes within it must be understood as quite intentional, and this consistently points to the subversion of the lofty origins of David’s house and the monarchy that he inaugurated. The great Near Eastern monarchies, ending in the Persian empire with its chaotic power struggles, its internal corruption, its massive destruction of peoples and property (of which Herodotus and Xenophon give us a vivid picture), are for this late author the negative models for his portrayal of the Davidic kingdom, and he clearly wants none of it. One cannot therefore use any of it to reconstruct the Davidic monarchy of the 10th century. And those parts of the Davidic tradition that are most often regarded as evidence of archival sources, the lists of David’s officials in 2 Sam 8:16–18; 20:23–26, and David’s family in 1 Kgs 3:2–5; 5:13–16, are simply the inventions of this late author.

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REFLECTIONS ON THE DISCUSSION: TEXT AND ARCHAEOLOGY

Lester L. Grabbe

The final chapter of volume 1 (Grabbe, ed., 2008) summarized the archaeological situation that emerged from the essays in that volume. Yet archaeology is only one source of the history of this period. The present volume has attempted to consider other sources: Egyptian inscriptions, other inscriptions, and the biblical text. Yet the field of the archaeology of Palestine does not stand still. Since the essays in volume 1 were finalized, some new data have already emerged (e.g. p. 234, below), but the main concern of this concluding chapter is to bring the data of the two volumes together and consider what the entire debate (not just that of vol. 2) tells us about the historical methodology of ancient Palestinian history.¹

The Archaeology, Inscriptions, and Culture of the Early Iron

The place to begin any history is with the primary sources. In this case, it is the archaeology (the artifacts and other finds from controlled excavations and surveys), the Egyptian inscriptions, and the few Palestinian inscriptions from this period. The archaeology does not generally tell us about individuals, whether kings, religious leaders, or common people. Even where we have finds associated with an individual, such as the human remains in a burial, we have no specific knowledge about the identity or life of that individual. What we do find are all important data about settlement and the culture of the region. R. MILLER surveys some of the general information about the Iron I, though his article mainly

1. The essays of volume 1 are referred to in the conventional way, by author–date–page number, as are other published works. The essays in the present volume are cited by the author’s name IN SMALL CAPS. Because E. A. Knauf has three contributions, they are indicated by “KNAUF: ‘Joshua’”; “KNAUF: ‘Judges’”; and “KNAUF: ‘Exodus’.”

introduces a project on the culture that is only beginning. B. A. Nakhai (2008) had also attempted to put village life of the Iron I in context. She argued that the diversified village life in Iron I was a response to socio-economic situations and led to differences in economies, skills, and social relations. This regional variation introduced a permanent element of instability that hindered unity when Israel developed statehood. On the potential importance of the copper trade in the development of the first state in the highlands, see “The Kingdom of David and Solomon” below.

We have few inscriptions from Palestine itself; there are more from Egypt, though they do not always tell us what we want to know. These are surveyed in GRABBE. Especially important is the light that they and archaeology throw on the settlement of the Philistines. The indications from pottery, the Medinet Habu and related inscriptions, and archaeology in general are that the Philistine settlement took place in the late 12th century B.C.E. (Ussishkin 2008; cf. Killebrew 2008). Another inscription, perhaps at the very end of our period, is the “*bytdwd* inscription” from Tel Dan. P. DAVIES deals with it in his study of the early kingdom of Judah. He argues that *bytdwd* probably did not refer to a kingdom or state of Judah alongside Israel; rather, the expression pertained to a dynasty “house of David,” possibly a reference to a vassal chiefdom of the kingdom of Israel. The kingdom of Judah does not exist in the extant text of the Tel Dan inscription. Whether there was a state centred on Jerusalem is unclear, but the biblical text indicates Judah in the 9th century was controlled by Israel: a “united monarchy” existed, under Omride rule. The Tel Dan inscription is important, but there are many questions about how it should be reconstructed (cf. GRABBE).

One live issue relating to archaeology is that of the “Low Chronology” (LC). I say this in spite of the fact that I have had more than one scholar approach me privately to assure me that the LC is dead—that no archaeologist believes it. Yet I keep coming across archaeologists who find supporting evidence for it or parts of it! The debate is complicated and involves evidence from a variety of disciplines. I have not personally come down on one side or the other (because I do not feel I have sufficient control of all the data to make a decision). But I do not believe the debate is over, whatever some archaeologists seem to think. Something may turn up tomorrow to decide the issue definitively, but I expect the discussion will continue for some time, with a slow accumulation of evidence pro and con until a proper consensus develops.

Use of the Social Sciences

An element within the recent changing scholarly approach to the history of ancient Israel is the use of the social sciences. To be sure, some of the great pioneers of study of the Hebrew Bible had made use of the social sciences available in their own time, including Alt, Noth, Albright, and some of their followers. But it was in the 1970s that social sciences began to find a major place in some studies, including in archaeology. Several contributors to the present volume and its predecessor focused on social scientific aspects of the question. E. A. Knauf (2008) brought up the statistical value of shipwrecks as an indicator of periods of prosperity and of the volume of international trade. These might have implications for periods of decline and recovery even in the Canaanite highlands, though the matter is complicated. But what we must recognize is the value of anthropological, sociological, and economic studies for the archaeology and history of ancient Palestine.

Looking at the other side of the coin, N. P. LEMCHE made some trenchant criticisms of how biblical scholars have misused the social sciences in developing theories about the history of Israel. To take one important example, the attempt to determine ethnicity by material culture is generally viewed as misplaced (see also the critique and bibliography in Grabbe 2007b, 18–21). As LEMCHE notes, material culture does not necessarily conform to ethnicity. Another tendency he criticizes is the propensity to choose one model from social anthropology and follow it without recognizing that it may be only one model among many in the social-scientific literature.

In my view, LEMCHE is right; indeed, I have argued this for some time. This is not because I regard the social sciences as unimportant; on the contrary, I believe that they are very important (as does LEMCHE), but they must be used with proper methodology (e.g. Grabbe 2001). One cannot take theory from social anthropology and apply it to biblical studies as if it were a fact—which is often done. Theories in the social sciences are challenged, contested, and tested just as are those in other disciplines. The tendency of students of the Hebrew Bible to take a social scientific theory and then simply use it as a model for a study in their own discipline remains a major weakness. There is nothing wrong with using such models, but they must be tested. Their ultimate value is in being a means of interrogating the data in one’s own field. (For example, see GRABBE on state formation.)

The Biblical Text

To repeat the statement in the previous section, any work of history must begin with the primary sources. The biblical text is clearly not one of these. Yet in spite of the conclusions of some of those labelled “minimalists”, the biblical text remains at the centre of the debate on the history of ancient Israel. To what extent can the biblical text be used in historical reconstruction? Scholars still give a variety of answers to this question, but the answers do not always arise from a careful study of all the evidence: theological and philosophical considerations sometimes seem to be the more important determining factor.

Among the essays in the present collection, the minimalist position is given by N. P. LEMCHE. He (along with some others) opposed even tackling the subject of the book, which is essentially the Iron I and Iron IIA. The reason was scepticism about finding anything in the text of value for history in this period (the value of archaeology and inscriptions is taken for granted). This is very much in line with the general minimalist approach: the biblical text can be accepted only when confirmed by contemporary sources. In that sense, LEMCHE’s article is consistent. Yet one can ask: How can you know whether the biblical text is supported until we make a careful investigation? To be fair, some were of the mind that they had already examined the text and found nothing of historical value in it (“We couldn’t see much happening that allowed for a new vista on the subject” [LEMCHE]). On the other hand, others of us were not so sure that we were in a position to pronounce a foregone conclusion on the matter—that there was still much work to be done before we would be in a position to draw conclusions about the biblical text. This is why we had the discussion, anyway, in spite of the minimalist reservations.

We cannot claim to have reached a position of sweeping conclusions, since the studies in this volume only skim the surface of the question. Yet they do address some important broader issues (such as the historical value of Joshua and Judges and also the David tradition) and also provide some significant examples that allow general comments about the text. What the essays here suggest is that the biblical text does not generally give reliable data for this period (LB to Iron IIA)—in that sense, they support the minimalist position—but there are exceptions; to put it another way, some reliable data are embedded in the text but much of the biblical narrative reflects the speculations and views of a later period.

Yet the key minimalist conclusion (that there was nothing of historical value in the biblical text) was not generally supported by the essays here. For example, KNAUF’s careful teasing out of authentic early material in Joshua and Judges seemed to meet with little disagreement. The early

data that he found amounted to much less than scholars of the past thought to find, even within German scholarship. Yet it did add up to a valuable set of data with which to supplement the archaeological and inscriptional information. The text is a problem and challenge—in that, the minimalists have it correct—but it is too early to write it off.

When it comes to the Saul and David traditions, they provide an interesting mix. As will be noted below, there is much late material to be found in them, but a case can be made that aspects of the Saul corpus are credible. The Davidic tradition is quite different from the Solomonic. It is interesting that a number of anti-David traditions are found in the Davidic complex. It is not just the Bathsheba episode. There is more than one rebellion (Absalom [2 Sam 14–19]; Sheba [2 Sam 20]). There is even the story of another person bringing down Goliath (2 Sam 21:19). Although the story of Solomon has criticisms of the king, they are those of the religious editor, but this does not explain some parts of the Davidic tradition. It all needs careful analysis, but the *prima facie* appearance is that among all the layers of compiling and editing there might be some usable data.

The Reconstruction of History in Iron I and Iron IIA

The biblical texts that cover the period of Iron I and Iron IIA are Exodus, Numbers, Joshua, Judges, 1–2 Samuel, and 1 Kgs 1–15. What is my evidence for listing these texts? They do not talk of archaeological periods. Yet the biblical narrative does give a sequence of events, leading up to the kingdoms of Israel and Judah in the Iron IIB: the exodus from Egypt, the wandering in the wilderness, the entrance into the land and its appropriation by force, the period of rule by “judges”, and the first kings of Israel and Judah. One can dispute details, but there seems no question that the compilers of the text would have associated it with the period which modern scholars call Iron I and Iron IIA. Thus, it is entirely appropriate to ask what these texts might tell us about the history of this period. In examining this question below, the history of Israel by Mario Liverani (2005 [Italian original 2003]) will be drawn on, as well as the essays in the volumes 1 and 2 (Liverani is a member of ESHM, although he was not present at the discussions of the essays printed here).

Exodus from Egypt

The first biblical “event” is the exodus of Israel out of Egypt. A tradition of having been in Egypt and coming out seems to be foundational in Israel’s memory (KNAUF: “Exodus”). It is attested in one of the earliest biblical books, the book of Hosea (2:17; 8:13; 9:3, 10; 12:2, 14

[ET 12:1, 13]. The possibility of Israelites in Egypt has been recognized and debated for decades because of evidence of Semites in Egypt through much of the 2nd millennium B.C.E. It might well be that Israelites were taken prisoner in fighting with Merneptah and later returned from captivity (KNAUF: "Exodus"). In any event, the central highlands and their inhabitants were under Egyptian control until late in the 12th century, and the removal of Egyptian control could be (according to attested linguistic usage) designated as "coming out of Egypt" (KNAUF: "Exodus"; Liverani 2005, 278–79). There is also the Egyptian name of Moses which suggests an early associated tradition.

Yet the biblical exodus tradition is clearly based on and inspired by much later events (KNAUF: "Exodus"; Liverani 2005, 270–82; cf. GRABBE). The most important influence is the return of Jews from Mesopotamia to Judah in the 6th and 5th centuries B.C.E., an event which was interpreted as a "second exodus" in the biblical tradition itself (Isa 51:9–11; Jer 16:14–15; 23:7–8). Yet there is also the influence from the Assyrian period, since references to Egypt often function as a metaphor for Assyria, and Assyrian deportation ends with a return just like the exodus from Egypt (cf. Hos 11:11; Liverani 2005, 278–80). Also, the biography of Moses has some remarkable parallels with the story of Jeroboam I (KNAUF: "Exodus"). The Moses story shows "growth rings" which indicate a development that drew on the Jeroboam tradition in order to develop the biblical life of Moses.

The Book of Joshua

If Israel believed that it had once been in Egypt but was now settled in its own land, it must have entered the land at some point in the past. The question of entry was not a burning one as long as Israel was a nation, but once it ceased to be a state, though continuing as a people under foreign rule, the matter of how it got the land was more important (KNAUF: "Exodus"). This is where the matter of what happened to the pre-Israelite settlers became a major question and had to be dealt with. The names of the people dispossessed were part of the "encyclopaedic" knowledge of the author and were anachronistic for the most part (Liverani 2005, 274–77; KNAUF: "Exodus"; Na'aman 1994). In some cases, they were known from contemporary Babylonian and Assyrian texts, where they had become traditional designations for the inhabitants of the Syro-Palestinian area (*Hatti* and *Amurru*). "Canaanite" was a designation first and foremost of the Phoenicians, though it could refer to anyone in the traditional area of Canaan. Some names were local designations or even artificial creations ("Perizzite" probably came from a word meaning "those who lived in the open country").

The tradition as it developed took on the concept of *hērem*—the inhabitants were proscribed and assigned to being driven out or, in many cases, to complete destruction. Yet this appalling image of genocide (from a modern point of view) was evidently a later development of the tradition, perhaps drawing strongly on the Assyrian practice of complete domination and savage destruction of cities and peoples that refused to accept the Assyrian yoke (KNAUF: "Exodus"). The inscriptions of the Assyrian kings left no doubt that their conquests were in the name—and honour—of their god Ashur. The redactor(s) of the Joshua tradition, for whatever reason, found this a useful model for Israel and Yhwh. As pointed out by KNAUF, it is good to know that this destruction of Canaanites was a paper exercise for the most part, but the bad news is that it became a part of the biblical tradition and could be invoked to justify some very horrific actions in history by appeal to the will of the deity.

It has long been known that the cities of Jericho and Ai were uninhabited during the LB/Iron I transition. These and other ruined sites evoked explanations and generated stories to account for their ruined state. This has been conventional in Hebrew Bible scholarship for generations of scholars. The settlement lists in Josh 13–19 may well be based on historical administrative units, but not necessarily early ones. For one example of lists (Josh 15:21–62; 18:21–28; 19:2–8, 40–46) investigated at length and determined to be from the time of Josiah, see N. Na'aman (1991, 2005). The concept of a unified conquest, once popular in scholarly circles in North America, has long since been abandoned for other explanations and theories that better fit the archaeological and other data (see the survey in GRABBE). This rules out the book of Joshua as a historical account. E. A. Knauf has been able to find a few passages that seem to embody genuine information relating to the LB, Iron I and Iron IIA (KNAUF: "Joshua"), but it is a rather meagre haul. Much of the narrative in Joshua looks like what Liverani (2005) calls "invented history" rather than "normal history." The author has created an ideal picture of an Israelite conquest, in contrast with the historical situation which was one of settlement in the hill country by mainly indigenous peoples.

The Book of Judges

The book of Judges is an interesting study because the surface picture may not be enormously removed from the situation in the later Iron I. At that time there was no overall governing power in Palestine, the Egyptians having lost control by about 1100 B.C.E. The five cities of the Philistines were perhaps the largest and most prosperous people, but they did not control the hill country or the northern valleys. Thus, it would not be surprising if there were conflicts between the Philistines and the

peoples of the hill country, and between the latter and other round-about peoples, ranging from the Transjordanian tribes to the Amalekites. The conflict-ridden scenario of small groups oppressing others or defending themselves from oppression, and a general condition of lawlessness and danger to individuals, could well have been the situation during this period of a couple of centuries.

Yet the indication from traditio-historical study is that only a few passages contain data from an early period (KNAUF: "Judges"). The model for the lawlessness and the conflict between local groups might just as easily be the Babylonian period, following the fall of Jerusalem, or the early Persian period before local Persian administration had taken firm hold (Liverani 2005, 292–96). Babylonian government of conquered areas seems to have been quite careless and even uninterested, in comparison with the earlier Assyrian or the later Persian administration. At a time when Jerusalem itself was uninhabited or only sparsely settled at best (Lipschits 2003, 329), it would hardly be surprising if lack of firm regional control left the local peoples to fend for themselves against raids from neighbouring tribes and city-states or even neighbours closer to home. Such a situation as found in the book of Judges could have pertained in Judah in the later 6th century B.C.E.

The book was not simply a creation of the early Persian period, however; its growth was much more complicated than this, with some its texts as early as the 7th and 8th centuries, though some as late as the 3rd century B.C.E. (KNAUF: "Judges"). But according to KNAUF the core of the book (the "Book of Saviours": 3–9, 12) arose not in the post-exilic period but in time of the fall of the Northern Kingdom of Israel and represents Israel without a kingdom and oppressed by foreigners. It was at that time that the "anti-monarchic" portion of the book grew up. Kingship had failed, and they needed a saviour who would not turn into a king. But the Judaean adaptation in the Persian period supplied the "proto-monarchic" voice found in many passages in the book.

Here and there, though, the book seems to have a memory of early events. The Song of Deborah has long been thought to be an early passage, and this is accepted (though it seems to be no earlier than the 10th century and was put in writing perhaps in the court of Omri [KNAUF: "Judges"]). The poem basically describes a conflict between the Sea Peoples (led by Sisera) and the tribes of Zebulon and Naphtali. Gideon's eastern campaign (8:3–21) may reflect the Midianite involvement in the Rift Valley copper trade from the 11th to 9th centuries (according to the LC). A conflict between the clan of Abiezer and a Midianite raiding party would have been possible anytime during this period.

The story of Ehud (3:12–30) has also been dated earlier, but there are mixed signs. The defeat of Moab by the Benjaminite clan Ehud was possible during the period of weak state power. This could have been 1100–875 B.C.E. but it could have been 724–716 B.C.E. Moab's conquest of Jericho was possible once Mesha had conquered Nebo (ca. 850–840 B.C.E.). Also, as KNAUF analyzes it, Ehud escaped down an Assyrian type of toilet (possible from 716 B.C.E. onwards). Elements of various periods thus make up the story, but it may well reflect an event of the late Iron I or Iron IIA. Judges 8:13–17 might be an oblique reference to the activities of Pharaoh Shishak who conducted a campaign that destroyed Penuel and Succoth (after which Penuel was rebuilt by Jeroboam I).

A reference to the battle of Qarqar also seems to occur (8:10). It is only a place name, but in this important battle of 853 B.C.E. a coalition of Syro-Palestinian powers opposed the Assyrian invader with some success. Strangely—or is it strange?—the battle of Qarqar is not mentioned in the Ahab story.² Judges 9 features Shechem and may describe tensions between tribe and town, perhaps David and Jerusalem or even Saul and Gibeon (KNAUF: "Judges"). It is unlikely to predate 1050 B.C.E. because Shechem was not settled between the late 11th and the late 10th centuries (at least according to the LC), though it was resettled as an Israelite royal residence.

The Kingdom of David and Solomon

Apart from my own survey (GRABBE) there was little speculation about the "United Monarchy" as such. It is interesting—curious, even—that references to the kingdom of David or the "United Monarchy" seem to have been most frequent in the archaeology volume (Grabbe, ed., 2008), in essays by Finkelstein et al.; Knauf; Mazar; and Steiner. Although there were different assessments of the archaeology for the period, the actual comments about Jerusalem in the 10th century were remarkably consistent; that is, none of those commenting on the question seemed to want to describe Jerusalem as remotely resembling the biblical picture of Solomon's city. In the present volume, those who discussed the biblical tradition relating to David and Solomon were generally negative about

2. The reign of Ahab, including the battle of Qarqar, is discussed in Grabbe, ed., 2007a. The battle of Qarqar is not mentioned in the account of Ahab, probably because the compiler did not have information on it. The Assyrians are not mentioned in relation to Ahab, even though they were very important in his reign. Possibly this knowledge was suppressed, but more likely is that this information was not remembered in the brief data that came down to the compiler from early sources.

the use of it for reconstructing the United Monarchy. There was not a systematic study of these traditions; however, a number of examples helped to comment on how the story grew to its present form.

M. BRETTLER gave only an example involving 1 Sam 24 and 26, but it illustrated a widespread tendency. He suggested that 1 Sam 24 knew and adapted the earlier 1 Sam 26, though in its present form the text of ch. 26 is, he believes, still far removed from the age and life of David. He concluded that, ultimately, these two chapters may not be used to reconstruct the history of the early monarchy. Their aim is to glorify the magnanimity and piety of David, while simultaneously condemning Saul out of his own mouth. A. G. AULD covered a greater amount of text, though not in detail; however, he concluded that much of the text grew not by incorporating new sources but by filling in gaps in the story from materials already available, in a midrash-style development. The David and Solomon narrative, for example, was created by drawing on the narratives of later kings. Although the accounts of Judahite kings and even Israelite kings were in many cases based on an actual court chronicle (cf. Grabbe 2006), this evidently did not exist as early as David and Solomon. Although individual researchers may not agree about specific texts, the two processes described by Brettler and Auld are not mutually exclusive but, rather, variants of a midrash-type of growth in the tradition.

Liverani (2005, 308–38) made the point of how the question of the monarchy was a matter of live and passionate debate after the fall of Jerusalem in 586 and for the rest of the 6th century B.C.E. This is shown by contemporary literature, such as Ezek 40–48, Haggai, and Zech 1–8. Yet it has also left its mark in the DtrH (e.g. 1 Sam 8). Many of the stories about David and Solomon are what the people of the “Deuteronomic age” would have imagined the United Monarchy to be like (Liverani 2005, 316). It is hardly surprising that in certain ways they are pictured as ideal rulers over an empire covering the whole of the Trans-euphrates region, as well as possessed of great wisdom, military prowess, power, and wealth. Stories reminiscent of fairy tales and the “Arabian Nights” found their way into the narrative. Yet other stories are told to their detriment, such as that of Bathsheba and Uriah the Hittite—hardly the product of any official record of the king’s reign (even if such a record existed).

In some cases, this growth made use of “encyclopaedic knowledge” for plausible data to help in expanding the narrative. As noted above, KNAUF (“Exodus”) and Liverani (2005, 274–77, 289–90) showed that lists of the various supposed original inhabitants of the land of Canaan were created by drawing on some knowledge of earlier and some contemporary peoples in the region but also even mythical figures.

J. VAN SETERS argued that the passages associating David with Cypriots and other Greeks were written long after the time of David. Although the Seminar discussion did not seem ready to settle on a particular date for this information (VAN SETERS argued for the Persian period), there was agreement that later traditions about Greek mercenaries were being incorporated into the David narrative. Thus, the view—widely believed in some circles—that David employed Aegeans among his body guards or in the temple was not supported. The story was enhanced—midrash-like—from external material.

It is thus clear that many aspects of the Davidic tradition are late or the product of literary creation. The tradition was developed and shaped and invented over a long period of time. Yet there are some aspects of the tradition that indicate some possible early memory. It has struck me that the lists of David’s “mighty men” have little parallel with other rulers in the biblical text. It is also interesting that one of these lists contains the alternate tradition about the slayer of Goliath, which is generally regarded as more likely than the Davidic account (2 Sam 21:19). This does not mean that these tales go back to the Iron I, but traditions which do not easily slip into known stereotypes can be signs of actual historical memory. It is here that some of the archaeologists (rather than the textual people) made suggestions about what history lay behind the Davidic story.

It is now worth repeating a couple of quotes found in my article (GRABBE) because of what they tell us about a possible developing consensus. First, here is a statement from I. Finkelstein (2001, 107–8):

...we may still be able to identify in them [the stories of David] the action of a local chieftain who moves with his gang to the south of Hebron, in the Judean Desert and in the Shephelah, far from the control of the central government in the highlands further to the north. David takes over Hebron, the second most important Iron Age town in the highlands of Judah and the centre of his theatre of operations, and then expands to the north and conquers Jerusalem, the traditional centre of government in the southern hill country. David, according to these stories, is a typical Apiru leader, who manages to establish a new dynasty in Jerusalem.

Yet the views of A. Mazar do not seem to be hugely different (2007, 164–65):

It is certain that much of the biblical narrative concerning David and Solomon is mere fiction and embellishment written by later authors.... I would compare the potential achievements of David to those of an earlier hill country leader, namely Labayu, the *habiru* leader from Shechem.... David can be envisioned as a ruler similar to Labayu, except that he operated in a time free of intervention by the Egyptians or any other foreign

power, and when the Canaanite cities were in decline. In such an environment, a talented and charismatic leader, politically astute, and in control of a small yet effective military power, may have taken hold of large parts of a small country like the Land of Israel and controlled diverse population groups under his regime from his stronghold in Jerusalem, which can be identified archaeologically. Such a regime does not necessitate a particularly large and populated capital city. David's Jerusalem can be compared to a medieval Burg, surrounded by a medium-sized town, and yet it could well be the centre of a meaningful polity. The only power that stood in David's way consisted of the Philistine cities, which, as archaeology tells us, were large and fortified urban centres during this time. Indeed the biblical historiographer excludes them from David's conquered territories.

Although A. Mazar's description might seem to be the most conservative, the end result was not greatly different from Finkelstein or from Steiner (2008). He interprets the Stepped Stone Structure as monumental architecture that was in place already by the 10th century (though others date it later: Steiner). He also supports Eilat Mazar's interpretation of the building above the Stepped Stone Structure as a building from the time of David (opposed by Finkelstein et al. 2007). Yet both Mazar and Finkelstein compare David with Labayu, the ruler of Shechem during the Amarna age who is associated with the *'Apiru*, at least by his enemies.

This comparison of David with the *'Apiru* is quite common these days. Exactly who first interpreted David as an *'Apiru* leader is not clear to me. This interpretation should perhaps not be surprising, of course, since 1 Sam 29:3 refers to David as a leader of *'Apiru* (*hārīrîm*). But the identification of David as an *'Apiru* leader goes back at least to Albrecht Alt and has been followed by a number of subsequent scholars.³ Yet Alt and others after him seem to limit this function on David's part to the time before taking the throne. The view that David continued in the role the rest of his life—that he was a latter-day Labayu even while ruling from Jerusalem—seems to be a modern one. It is clearly one different from that assigned to him by the biblical tradition in which he is the founder and ruler of an Israelite empire. Some reasonable conclusions about the historicity of the tradition are the following (GRABBE):

- The tradition recognizes that David was not the first king.
- Saul came to the throne probably as a military leader by popular acclaim (1 Sam 11:1–15), whereas the prophetic tradition that the king was subject to Samuel's choice and censure (1 Sam 9:1–10:16, 23; 13:2–14:52) is unrealistic.

3. Alt 1953a, 170; 1953b, 37–38. Dependent on Alt are Greenberg 1955, 76 n. 73, Mendenhall 1973, 135–38, and McCarter 1986, 120–21.

- The apparent boundaries of Saul's kingdom (2 Sam 2:9) is reasonably in line with the natural and demographic resources in Cis-Jordan.
- A strong link is made between David's rise and Saul's court, but much of this looks like a deliberate attempt to legitimate David as king from a variety of angles: anointing by Samuel (1 Sam 16:1–13); armour-bearer in Saul's court who plays the lyre for him personally (1 Sam 16:14–23); slaying of Goliath (1 Sam 17); marriage to Saul's daughter (1 Sam 18:17–27).
- Although David is mainly presented as a positive—even ideal—figure, several significant “sins” or weaknesses maintain a place in the tradition, as well as rebellions against his rule. Also, some alternate traditions are preserved, such as another slayer of Goliath (2 Sam 21:19).
- Contrary to expectations David does not build a temple (though a strenuous effort is made for him to do everything short of the actual building).
- Both Saul and David were mainly military leaders.
- The text itself does not suggest an extensive administrative apparatus in the case of either Saul or David.

Yet it is not only the more conservative who believe that Judah began the process of state formation. Although KNAUF sees a minimum of historical data in the biblical text in his essays in the present volume, he argues that the situation at the time favoured the beginning of state formation with Judah, viz., the copper trade (Knauf 2008, 83). In the 10th century the crossroad through Benjamin was more important at first, suggesting that the Israelite state formation started in the Benjamin–Jerusalem area and spread to Shechem only later. In the 9th century, southern economic dominance was supplanted by northern, Phoenician dominance (and Cyprus copper). This is attested not just in the biblical account but on the ground: Gibeon (and Jerusalem to a lesser degree) flourished while Shechem lay in total eclipse during the late 11th and early 10th centuries.

Conclusions

In spite of the reservations of LEMCHE and others, this has been an exciting and interesting study. As has been clear, it has in part confirmed the views of the minimalists. When LEMCHE states, “The Old Testament only distorts the interpretation of whatever information we have from the Early Iron Age”, he is of course quite right—for the early Iron. Yet I

cannot share his pessimism about scrutinizing the biblical and other traditions for possible historical information. As a historian I see my job as one of trying to find data wherever it may be found, whether in rich soil or on stony ground. In any historical period—certainly, in any period of ancient history—we have good sources and bad sources, or at least better sources and worse sources. Looking at the Bible for possible information of value is not different from a classical historian looking at traditional Greek sources. Some, such as Homer, give us little of direct use, but there is agreement that there are certain reflections of the Mycenaean age in the Homeric poems. Livy's account of early Rome is also problematic for the historian, but it cannot be dismissed without consideration.

The backbone of any history of this period has to be archaeology and contemporary texts (primarily Egyptian ones). But archaeology allows us to reconstruct only prehistory, while the contemporary texts fill in the picture only here and there. From one point of view, the contribution of data from the biblical text is hardly worth the effort, but from another perspective every datum is precious information that fills out the picture in a way not possible from other sources alone. Thus, although the Joshua narrative itself has little to do with historical reality in Palestine in the LB or Iron I (as archaeology shows), there are a few bits of information from the 13th to the 9th century that add to our broad picture. Knowing of a battle between the Sea Peoples and the inhabitants of the northern valleys (Judg 5) is a valuable addition to our knowledge of the Iron I. The reconstructed picture drawing on these data, carefully teased out by KNAUF ("Judges"), is somewhat speculative, as are the pictures based on archaeology and on all the available primary sources.

My own analysis came up with the following conclusions about the biblical text (GRABBE):

Biblical Text Confirmed

- The name "Israel" (Merneptah), showing some sort of entity in existence.
- Existence of Philistines in the coastal plain.
- Long-term division between Judah and Israel.
- David the founder of a dynasty in Judah (Tel Dan).

Biblical Text Not Confirmed but May Be Right

- Some part of Israel having been in Egypt.
- Diverse origins of Israelites (cf. Ezek 16:3).
- Settlement of those who became Israelites initially in the hill country (so Judg 1).

- Saul as the first real king of Israel.
- Jerusalem made the capital of David's territory (chiefdom, city state, or incipient state).
- Temple in Jerusalem built by Solomon.

Biblical Text Most Likely Wrong

- No large-scale emigration of foreigners from Egypt in the LB age.
- Most of those making up later Israel likely indigenous, not immigrants.
- No unified conquest of the land as pictured in Joshua.
- No evidence found for a magnificent kingdom for Solomon, in spite of some recent attempts to reconstruct a splendid city of Jerusalem in the 10th century.

As LEMCHE says, it is possible to write a history of Israel without the Bible. Using archaeology and other primary sources alone will give us the same crude outline as can be found for any prehistoric culture. But why should we stop there if we can go further? After all, as LEMCHE acknowledges, Liverani made use of the Bible in his "normal history".⁴ He wanted to go "beyond the Bible"—to get away from history as simply a biblical paraphrase (a biblical "hypertext", as LEMCHE puts it). This is also my aim. I have argued strongly that we must begin with the primary sources of the archaeology and the inscriptions; nevertheless, our history seeks to go *oltre l'archeologia e le iscrizioni*. There are times when this is not possible—when the biblical text is too late or problematic to give us additional information—and there are times when the extra data it supplies are very minimal. As the essays in the present volume indicate, this is often the situation in the period of the Iron I and seems also to be the case in the Iron IIA. Yet sometimes we can enrich our historical reconstruction with biblical information, and when we move into the Iron IIB and later, this becomes possible more often, and the picture—in fits and starts—becomes gradually fuller. At that point, the biblical text ceases just to "distort" (to use LEMCHE's word) but to give us valuable independent information. As I have argued elsewhere, the text generally seems to become more trustworthy, the later the period (Grabbe 2007b, 222–23). In the latter half of the 7th century there are times when this history can be traced almost year by year. But in the

4. The name of the original Italian edition of Liverani's book (*Oltre la Bibbia*) does *not* mean "without the Bible," as those whose Italian is much better than mine have confirmed. It means "beyond the Bible," which is an aim completely within my instincts as a historian.

crucial period from 1250–850, sadly, there is much we do not know, and the picture must still largely depend on archaeology and the few Palestinian and other inscriptions presently known.

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APPENDIX:
EXODUS AND SETTLEMENT*

Ernst Axel Knauf

Departure and Arrival, Land Promised and Occupied

That Yhwh led Israel out of Egypt belongs to the foundational stage of the biblical tradition, since Israel is usually spoken of as the people of Yhwh and Yhwh as the God of Israel. The view that Yhwh then led Israel into Canaan is, however, no tradition but a theology based on the exodus tradition which is not attested prior to the 8th century B.C.E. The theology of the 7th and 6th centuries B.C.E. goes a step further in making the land a grant from God. In light of a German-speaking scholarly tradition that has spoken of the “*Landnahme* (land acquisition) of the Israelite tribes” for more than a century, it cannot be too strongly emphasized that any “taking of (the) land” is as good as absent from the Bible. On the reception side of the land granting we always find Israel as the people of God and not an assembly of belligerent tribes.

In countless passages in which God says (or which say of God) that he will give or has given Israel the land, only two opposing instances occur in which it is stated that Joshua “occupied” the land (Josh 11:16, 23a; both from the end of the D-composition, covering Exod 2 to Josh 11*). In Num 21:26 and Deut 3:8; 29:7 the Transjordanian land of Sihon and Og is “taken”; in Judg 11:13 the Ammonites accuse Israel of seizing the opportunity to “take” the land from them (which Jephthah disputes in Judg 11:15); finally, Assyrians and Babylonians “take” parts of Israel or even the complete Transeuphrates region (2 Kgs 15:29; 24:7).

* The original German version of this study appeared in E. A. Knauf, *Josua* (ZBKAT 6; Zurich: Theologischer Verlag Zurich, 2008), section 3 of the introduction. The English version was translated by Lester L. Grabbe and slightly revised by the author, including the removal of the original paragraph numbers.

In the history of academic debate “the land acquisition” terminology is based on a false analogy of the misunderstood “land acquisition” by German tribes at the time of the Roman Empire. The Germans came into the empire (and were provided with land) because the Romans had a population deficit and invited the tribes as “guest workers” (mainly for military service). The ancestors of Israel, in contrast, came from nowhere: they were for the most part already there, inhabiting an area in which their descendants later created the states of Israel and Judah.

The Departure from Egypt

It is probable that in the first half of the 12th century a group of refugees from Egypt arrived in Canaan. As descendants from Israelite prisoners of war, which Pharaoh Mer-en-Ptah had deported before 1208 B.C.E., they found their way back and reported, “The God Yhwh led us out of Egypt.” This tradition of a small minority was received by the tribes, however, and passed on, developing from the 12th to the 9th century in the Samarian hills and in Gilead; for in some sense all these tribes likewise came from Egypt—namely, the Egyptian province of Canaan where they were settled.

In the 17th to the 15th century B.C.E. the urban culture of Canaan collapsed. The inhabitants of the hill country for the most part abandoned the city and village mode of life and carried on agriculture and herding in tent-dwelling family units. The coastal region, the lowlands of Canaan, which retained a remnant of urban culture, came under Egyptian rule in the 15th century as the “province of Canaan.” In the course of the 13th century the Pharaohs sought to bring the hill country and Transjordanian region under their control. In the meantime, bands of economic refugees from the increasingly crumbling Canaanite cities had formed themselves alongside the tent-dwelling farmers and herders. These dropouts were called ‘Apiru, from which comes ‘ivri, “Hebrew.” The collapse of agricultural production in the coastal region compelled those living in the hill country to move from extensive to intensive agriculture. The growing population density necessitated political coalitions between clans (i.e. tribes), because the mobility of the small group no longer provided adequate protection. A tribe (more often settled than nomadic in world history) is an alliance of groups of different origin for mutual defence. On the basis of the genealogical arrangement of subgroups the tribe agrees on a tribal ancestor (fictive, as a rule).

At the end of the 12th century, with the disappearance of the last Egyptian outposts from Canaan, all the inhabitants of the hill country

could say of themselves—for different reasons with but equal justification—“Our God has led us out of Egypt.” The Exodus creed may have entered the state cult of Israel by the time of Jeroboam I (end of the 10th century) or, more probably, Jeroboam II (middle of the 8th) (1 Kgs 12:28). Jeroboam I had been an exile in Egypt before he became king of Israel (1 Kgs 11:26–40; 12:2) and probably came to the throne with Egyptian backing. Therefore, not only is he the worst candidate for the role of cult founder based on the Exodus tradition, but also an unlikely candidate for a founder of the cult of Bethel, because Bethel was disputed territory between Israel and Judah during 925–875 B.C.E. and changed hands many times (cf. 1 Kgs 14:29; 15:26–34). Jeroboam II, on the other hand, consolidated a kingdom that his ancestor Jehu had grabbed through a military coup against the Omri dynasty (2 Kgs 9–10). This dynasty, together with Phoenicia and Aram-Damascus, had benefited from favourable and stable Egyptian “supervision.” Thus, also in the 8th century B.C.E. (and once again after 604 B.C.E.), “liberation from Egypt” was an experience of the recent past.

The Egyptian name of Moses argues that this concept was probably bound up with the Exodus tradition from the beginning. His biblical “biography,” however, shows growth rings. In Exod 2–14 it shows parallels to the biography of Jeroboam I. Both revolted against exploitation and had to flee abroad (Exod 2:11–15 // 1 Kgs 11:26, 40), and both triumphed finally over their pursuers (Exod 14 [D] // 1 Kgs 12). Here Jeroboam I is a model for Moses. In the 7th century, under Assyrian dominance, the story of “the baby in the bullrushes” (Exod 2:1–10*) was added to the Moses biography, because at that time it emanated from the Sargon I legend which had been picked up and disseminated by the Assyrian Sargonid dynasty (the descendants of Sargon II) as part of their propaganda.

The Entrance into Canaan and the Gift of the Land

As long as Israel was a state and the people lived in their own land, the “entrance into Canaan” would not have been picked up by itself as a major theme, because the final part of the Exodus tradition was present reality (cf. Deut 26:9: “Yhwh brought *us* here and gave *us* *this* land”). The Exodus tradition was thoroughly compatible as political theology with the cosmological conviction that Israel had had its portion of land assigned to it from creation (so Deut 32:8–9 in the Greek Bible, and in the late 9th-century king Mesha in his memorial inscription: “The people of Gad lived in the land of Ataroth since the beginning of the world”).

When Israel ceased to be a state after 724/720 B.C.E., yet survived as a land and people by this name along with their ancient deity, Egypt, besides being a perfect example of an imperial political power (which it remained to connote in the tradition), at the same time turned into the land on the Nile, and Eretz Israel—no longer the territory of a kingdom—became the source of livelihood given as a gift by God to the Israelite farmers. This stage of the tradition's development can be found in Hos 2:16–17; 9:10. In light of the time of disaster in Israel's failure as a territorial state, the wilderness became the prehistoric meeting place with God; from this utopian site a new beginning in the land would be possible. Hosea does not yet speak of a conquest of Canaan but only that God brought Israel into his land. The book of Hosea was compiled in Bethel after 720 B.C.E. from various prophetic pronouncements from the time of the state of Israel's downfall, in order to extract lessons from the past and to lay the groundwork for a new beginning under Assyrian rule. At this time the promise of the land to the patriarch Jacob took on a new meaning, because its validity (unlike the intervening national Exodus tradition) had not been placed into question by the Assyrian dominance. It is the achievement of the compiler and tradents of Hosea to have once again "denationalized" the Exodus tradition and to have turned it into a prophecy, so that it remained an option for Israel (Hos 12:13).

The Gift of the Land as a Command to Conquer It

Now that the Exodus tradition was understood to say that Israel had come out of the land of the Nile and into the land of Canaan, the question arose as to who had previously lived in Eretz Israel and what had happened to these earlier inhabitants. The "Joshua chapter" of the Moses–Joshua narrative answered this question at the close of the 7th century. The Assyrian overlordship had already collapsed, but Assyrian theology was behind two serious concepts, which were still evolving. Just as the Assyrian king concluded treaties with subordinate kings and vassal tribes, in which their existence was guaranteed by Assur in exchange for the observance of a list of duties, so the relationship between Yhwh and Israel was now formulated as a vassal treaty (Hebrew *b'rit*, usually but inappropriately translated as "covenant"). Israel received its land from Yhwh as a fief and must therefore keep his laws and statutes (this theological construct forms the basis of the book of Deuteronomy). Just as Assur, the (Assyrian) high god, had commanded the king of Assyria to subjugate the rest of the world, so Yhwh now commanded Israel (and Judah) to make his own land subservient to him. Conquest by divine

decree legitimated possession of the land and sovereignty—at least, until the next conquest. The prior inhabitants can submit (Josh 9*)—or they die in battle (Josh 6* and 10*). It is probably not superfluous to emphasize that this "wishes past," under the influence of Assyrian imperialism, did not take place in historical time.

One must, however, be aware of the fact that two concepts which have the same roots in mental history (like the Joshua expansion of the Exodus tradition and the "covenant theology" of Deuteronomy) need not always and absolutely be taken for works of the time, out of which arose its basic concept, or indeed for parts of a single coherent work. If the concept of a vassal treaty of the Assyrian type between Yhwh and Israel, as found in Deuteronomy, was already formulated in the 7th century (which is not certain), then until the fall of the kingdom the king (and not the people) would in any case have been the human treaty partner (as is to be expected in the sphere of ancient Oriental kingship). The "demotic" concept of Deuteronomy consequently presupposes the downfall of the state of Judah, in contrast with the Exodus–Joshua tradition (it is not to be excluded that Deuteronomy contains isolated laws and traditions that can be older).

That the expansion of the Exodus tradition by a conquest report was more likely revolutionary rather than an organic growth of tradition is perhaps indicated by the fact that Moses, bound up with the departure tradition, no longer led the conquest but was replaced by Joshua. Jewish Hellenistic traditions of the 3rd century B.C.E. already (or still?) knew a peaceful entry into the land—under the leadership of Moses (preserved in Strabo, *Geog.* 16.2.35–36).

"The Original Inhabitants"

Since Israel now needed not just to immigrate into the land but also had to conquer it, the question arose: Conquer it from whom? That was an academic question, a result of theological theory building at the time. It was answered by the background of contemporary antiquarian knowledge.

In the Moses–Exodus narrative the original inhabitants are called "Amorites" (Josh 10:5–6, 12). *Amurru* conjures up a prior history reaching back into the 3rd millennium B.C.E., but was hardly known to the biblical sages in its full complexity; they were familiar with the Assyrian and Babylonian usage of the 1st millennium B.C.E. in which "Amurru" simply is Syria between the Euphrates and the Nile. Babylonian was one of the administrative languages under Assyrian, Neo-Babylonian, and

Persian rule. In Jerusalem the relevant documents (or propaganda texts) were known in the 7th to 4th centuries at least in their Aramaic version (Aramaic was likewise an administrative language used throughout the empire). The biblical borrowing of traditions and terms from cuneiform literature is to be traced back neither to memory from the 2nd millennium B.C.E. nor to the readings of the exiles in Babylonia, but once again to the Israelite and Judahite administrative and school activity.

To name the inhabitants of a territory within the great region of Amurru “Amorites” is not very original, anymore than calling the citizens of Belgium “Europeans.” Only once does Amurru refer to the specific territory of a tribal kingdom during the Late Bronze Age (Josh 13:4). It appears that all other ethnonyms merged into the tradition with the Hexateuch redaction or later redactions of the book of Joshua.

Also, “Canaanite” had a double meaning. Already in the 2nd millennium B.C.E. it referred specifically to the inhabitants of the Phoenician coastal cities (Josh 5:1; 13:4?), and at the turn of the 1st millennium B.C.E. came to denote the non-Israelite inhabitants of the “Canaan” of Num 34:1–12; Josh 7:9; 13:3; 16:10; 17:12–13, 16–18.

At the end of the development of this “historical ethnography” stands a veritable catalogue of peoples that runs through the complete “Decateuch” (i.e. Genesis to Ezra–Nehemiah, without Chronicles), from Gen 10:15–17 to Ezra 9:1:

He will certainly drive out before you the Canaanites, the Hittites, the Hivites, the Perizzites, the Gergashites, the Amorites, and the Jebusites. (Josh 3:10d)

It was when all the kings across the Jordan, in the hill country, in the Shephelah, and in all the coast of the Great Sea opposite to Lebanon—the Hittites, the Amorites, the Canaanites, the Perizzites, the Hivites, and the Jebusites... (Josh 9:1)

the Canaanites in the east and the west, the Amorites, the Hittites, the Perizzites, the Jebusites in the hill country, and the Hivites under Mt. Hermon in the land of Mizpah... (Josh 11:3a)

in the hill country, the Shephelah, the Arabah, in the slopes, in the desert, and in the Negev, the Hittites, the Amorites, the Canaanites, the Perizzites, the Hivites, and the Jebusites... (Josh 12:8)

the citizens of Jericho fought against you—the Amorites, the Perizzites, the Canaanites, the Hittites, the Gergashites, the Hivites, and the Jebusites... (Josh 24:11c)

“Hittites” were the inhabitants of the land of Hattu (once more, a word from the cuneiform encyclopaedia); that was the Hittite empire in the 2nd millennium B.C.E. and its Syrian possession in northern Syria, with its descendant Luwian–Aramaic states at the beginning of the 1st millennium. In the Neo-Babylonian and Persian period “Hattu” had become a synonym of “Amurru.”

The “Perizzites” are “Boers” (Hebrew *p̄erizzî* “inhabitants of the countryside”), and the “Jebusites” were probably constructed rather than being historically attested as inhabitants of pre-Israelite Jerusalem. Perhaps “Jebusi” was the name of an old pre-Israelite Jerusalem family or even the leading family of the time when David took the city (2 Sam 5:6)? In Jerusalem’s genealogy Ezek 16:3 does not know of any “people” called Jebusites. Gergashites lived in Asia Minor in the 2nd millennium B.C.E. Hivites (“sons of Eve”) are an otherwise unknown group in Lebanon in Josh 11:3 and 12:8, but in 9:7 and 11:19 the inhabitants of the Gibeonite Tetrapolis inside Benjamin. Most likely the tribal name was the self-designation of these non-Benjaminites in Benjamin. According to Gen 10:15–19 the name had come into the list of original inhabitants and from there into Lebanon (Josh 11:3; Judg 3:3; 1 Sam 24:7). Finally, the ubiquitousness of the Hivites documents the last unresolved biblical question about the exact borders of Canaan.

For an English audience the list of natives could be read: “Before the British people, the Celts, Romans, Picts, Normans, Saxons, and Leprechauns lived in England.”

The Eradication or Expulsion of the “Natives”

The D-composition threatens the native inhabitants with annihilation through the “ban rite.” The relevant Hebrew word *hērem* is properly declined in Josh 7 through all aspects of its semantic range. In the pre-Assyrian period the “ban rite” of total annihilation was performed in honour of the divinity in individual cases (vowed before especially difficult conquests and in the face of great danger and enforced after victory), for example, by King Mesha with regard to the Israelite city and fortress of Nebo (cf. Num 21:2). The ban is a true sacrifice, since the spoils of war were the only hope of profit for those taking part in the campaign, as compensation for their civic loss of earnings. From the 7th century the Assyrian practice of war (or propaganda), “submission under the cosmic rule of Assur or death,” was equated with the concept of *hērem* (2 Kgs 19:11). If the concept of “annihilation” of the earlier inhabitants likewise had Assyrian roots, its use in the context of

D-theology was already an archaism, hearkening back to the practice of a previous era (therefore, what once constituted the “ban” must be explained in detail in Josh 7).

Submission is no option for the D-theology, only expulsion or extinction, because of fear of contamination of the new, true religion. The following passages speak of the ban or annihilation: Josh 2:10 (Sihon and Og); 6:18, 21; 8:26; 10:1, 28–40; 11:11–12, 20–21 (according to the speech of Moses in Deut 7:2–26 and the speech of God in Deut 20:17); those describing expulsion are Josh 24:12, 28, with reference to Exod 23:28–31; 33:2; 34:11. Deuteronomy 33:27 combines expulsion and annihilation (like Esarhaddon, who according to his own statement imposed heavy taxes on the Egyptians—after he had killed them all). That the *hērem* texts do not yet belong to the base narrative, but only appear with the D expansions, is shown by their theological horizon and especially by the literary stratigraphy of Josh 10 (10:28–39 forms a series of additions).

The good news: the genocide of the native population occurred only on paper, just as this, so to speak, itself led a purely encyclopaedic existence. The bad news: because it now has a place in the Bible, it consequently took on a disastrous history of actualization, especially in Christianity. The half-good and half-bad news: in the contemporary context of D the expelled or the struck-down aboriginal inhabitants—“Canaanites, Amorites and Hittites”—were none other than the Benjaminites who remained in their land, who at first remained true to their old ancestral religion in opposition to theological innovations of the returnees. As in many other cases (e.g. the “enemies of the reconstruction of Jerusalem in Neh 2–6) an inner-Jewish conflict was projected onto external foes.

Peace Treaty with the “Natives”

The Rahab story (Josh 2, together with its end in Josh 6) and the “list of Gibeonites” (Josh 9, elaborated version of the Hexateuch redaction) are continuing narratives (on the basis of P’s theology) in which Joshua does what he can in order *not* to annihilate the native inhabitants. The narratives are creative exegesis in the sense of “Not everything that stands in the Torah must always and forever be carried out by everyone.” This narrative goes further than the downgrading of the command of annihilation to one of expulsion in Josh 24 (and Exod 23), but perhaps continues the theology of the D-redaction closing (Josh 11:23), which if rightly understood here regards the annihilation command of Deut 20 as

completely fulfilled with the final conquest of the land. The “returnees” in Jerusalem took control in the province of Judah in terms of D-theology, and consolidated their place through the integration of those who remained (and their neighbours) in terms of P and Hexateuch theology—compromise after confrontation.

Preserved Memory—Deleted Time

The initial and end signals of the book (Josh 1; 10:40–42; 11:23; 18:1; 21:43–45; 23–24) point on the whole to a central theme, which is in line with theology of P, and to a subsidiary theme which carries D forward. In the subsidiary theme Joshua is a station on a linear historical narrative, which begins with creation and ends with the rebuilding of Jerusalem and of the temple in Ezra–Nehemiah (or with the return of Elijah, God’s commencement of his eschatological kingdom and the erection of a heavenly Jerusalem in Mal 3:22–24). Israel’s failed history under its kings is deleted in the central theme. The end announced by the prophets (Amos 8:2; Ezek 7:2–6) lies behind us (this prophecy is cited by P in Gen 6:13). In their inscriptions, Assyrian kings compare the ruins they left behind with the “rubble heaps from the flood”: in the biblical primordial account those who survived the Assyrian-Babylonian flood were blessed in Noah and replenished the earth anew. In the diaspora as in Eretz Israel they leave Egypt anew each Passover, receive anew at each Shavuot (the forerunner of Pentecost) the Torah on Sinai—a point outside time and space, where neither birth nor death occur (Exod 38:26; Num 1:46)—and with each Sukkot and each Simchat Torah confirm anew the treaty of Josh 24 (Neh 8:17). With the new beginning, which the community of the Second Temple made with the Torah, will come a new beginning for the community time and again. It does not go out to conquer the world but to take possession of its own garden and cultivate it. This is blessing, salvation and grace enough.

The cycle of festivals has been detached from the cult and temple and repeats now the collective memory in the yearly cycle that has become a book. The Torah is the myth of Israel (R. G. Kratz), which remembers and continually brings afresh to memory timeless legends of origin for its contemporary existence.

With the new beginning the “evil”—the failed time of the First Temple—is deleted, though certainly not the memory of it. P also knows that kings of Israel and Judah had once existed (Gen 17:6, 16; 35:11); the Hexateuchal end of Joshua (24:20) is also not silent about the bad end of the history of the kings. Israel’s failed and false history has passed—but it should not be forgotten, lest the error should once more be committed.

If, drunk with sight of power, we loose
 Wild tongues that have not Thee in awe,
 Such boastings as the Gentiles use,
 Or lesser breeds without the Law—
 Lord God of Hosts, be with us yet,
 Lest we forget—lest we forget!

—R. Kipling, "Recessional"

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